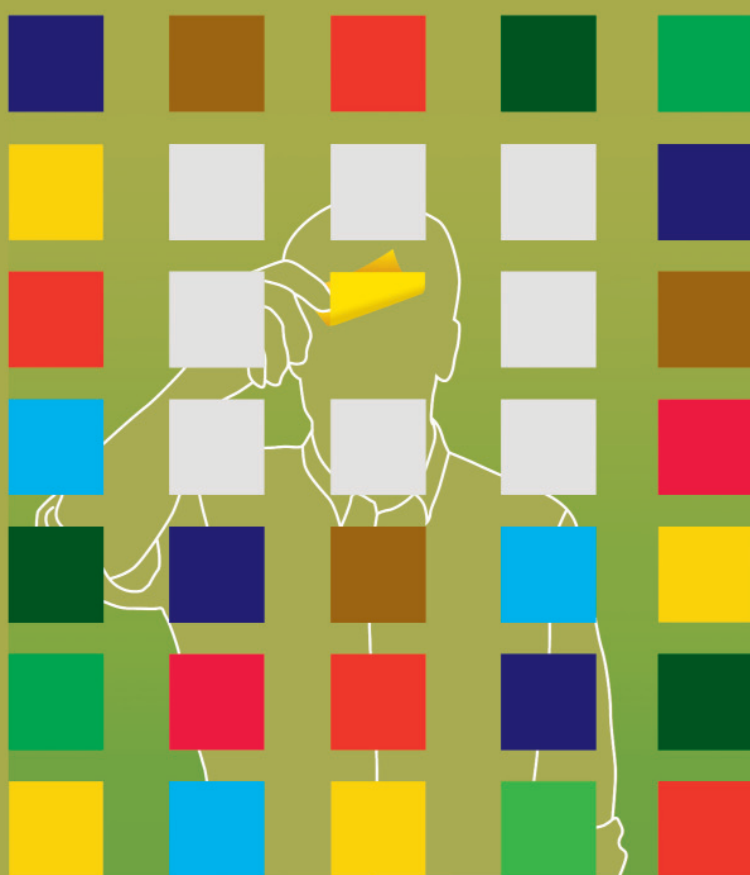


Readings in Methodology

African Perspectives



Edited by
Jean-Bernard Ouédraogo
Carlos Cardoso

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This book is a result of CODESRIA's Methodological Workshops

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African Perspectives

Edited by
Jean-Bernard Ouédraogo
Carlos Cardoso



Council for the Development of Social Science Research in Africa
DAKAR

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The Council for the Development of Social Science Research in Africa (CODESRIA) is an independent organisation whose principal objectives are to facilitate research, promote research-based publishing and create multiple forums geared towards the exchange of views and information among African researchers. All these are aimed at reducing the fragmentation of research in the continent through the creation of thematic research networks that cut across linguistic and regional boundaries.

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To Sémou Pathé Guèye

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Introduction: Questions of Method

Jean-Bernard Ouédraogo & Carlos Cardoso

All truth is simple: isn't this a double lie?

Bringing something unknown to something known lightens and reassures, and, moreover, provides a feeling of power. First principle: any explanation is preferable to a lack of explanation. Given that it is only a matter of ridding oneself of anguishing representations, we do not look too closely to find the means to get there: the first representation by which the unknown is declared known makes us feel so good that we hold it to be true.

Nietzsche

From the beginning, the principal reason for the organization of a series of methodological workshops for young researchers was the fact that no body of knowledge can be established if the procedures on which its knowledge is founded are not clearly established and mastered. And given that CODESRIA, as a pan-African institution, has as its main mission the promotion of a social science on Africa produced by Africans, such an objective is based on the certainty that progress in Africa can only be achieved by a rigorous intelligence of social realities on which it will thrive. This volume is, therefore, an assemblage of some texts produced out of those methodological workshops organized by CODESRIA since 2003. Naturally, the primary threat capable of compromising this mission could come from the approximate or erroneous exercises of collection procedures, theoretical treatment and presentation of social practices. Yet methodology is, unfortunately, often perceived as a tool box used in a fetishistic, mechanical and standardized way for which the researcher is thought to maintain a distant and deceptively instrumental relationship in the guise of intimacy. Moreover, this weak mastery of method – a dominant tendency today – seems to reduce the processes of scientific discovery to simple standardized procedures. Starting from such a limited methodological perspective, society becomes an extremely simplified reality, informed by effective prejudices imposed by a social order without which the

action so sanctified by the prevailing pragmatism would have no real political and ethical foundation. We understand that such a fusion of the quest for knowledge and transformative action considerably compromises the success of the two related moments; it is clear that the suppression of the distance, indispensable to scientific objectivization, renders problematic any research which is instructed by reason of the secret truth of the social world. With respect to action, its effectiveness is limited by the urgency which commands its implementation. This confusion probably comes from an approximate knowledge of the founding principles and conditions of implementation of the two terms that are improperly and simultaneously confused: research and action. The imperative of this direct action becomes the *credo* of all thought on social life, subjected by interventionists to an immediate and required correction. The break with such a tendency proves indispensable to the theoretical and practical construction of a scientific rigor which should always be at the root of the progressive unmasking of hidden aspects of the social world. We should immediately note that this encouragement towards reflection on methods does not mean, in our view, an acceptance of methodologism and theoricism which is associated with it; these two existing tendencies in social research insidiously dominate and rigidify the approach of the researcher by giving methodological systems a status which keeps them distanced from specific problems that the construction of the subject poses. A technical sovereignty of method develops as a simple sophisticated manipulation of indices and empirical observations. It is also indispensable to keep one's distance from "anarchistic," poorly monitored methods in order to adopt a measured methodology, which considers both the theoretical construction of the subject and of the necessary adaptation of research techniques.

However, the question of methodology would remain poorly posed if it was considered only with respect to its technical aspects, because this technicity itself cannot be understood except if it is within a definition that is given for science. The understanding of this exposition of science, such as it has been historically constituted, puts into perspective the role of the stages of the conquest of a certain knowledge of the social world and allows us access to legitimized knowledge as central social issues. Indeed, the preliminary of all use of methodologies is a clear delimitation of epistemological acts which have the goal of discovery, an objective which is always socially defined. But we should also remember that the execution of epistemological acts obeys a logic of organization, the omission of which leads to a lack of control of the research process and seriously compromises its effectiveness. In all logic, observation depends on the theoretical construction of the subject, which happens only after a series of breakaway operations. Seen from this perspective, the question of methodology becomes more complex and its resolution requires a reflexive return towards

unresolved problems, perhaps not yet raised as such in the young African social sciences, but its persistence limits the performance of acts of knowledge.

Regardless the perspective from which we consider methodology, a first series of questions is essential: what is science? In what conditions should its exercise be considered legitimate and effective? This double interrogation leads us directly to the heart of the “science” system and sheds some light on the tendencies which govern it. Faced with these formidable questions, the weight of history, of the thought of the Ancients, imposed by the insistent shadow of progress, has deeply marked modern science by the power of its ambitions, and the requirements of progress lead to debates in attempting to order a limited vision of this unusual human practice. We can, nevertheless, assert that modern science is constructed on positivism which has, in fact, become apparent as the expression of the triumph of reason over metaphysics. But then, if the power of the science of nature as an essential factor in the modern world facilitates the everyday nature of men, thanks to techniques that it provides them, there is no reason not to accord the same degree of scientific nature to the social sciences. And, since the era of Quételet and Durkheim, the ambition of this science of societies was precisely to impose its rationality as an equivalent to the recognized causality of the natural sciences. This fundamental question of the logical practice of sciences, clearly posed by Pierre Bouda, retraces the universal scientific adventures that the critique of Wittgenstein revisits; this exercise allows us to situate the origin of his epistemological meditations and his disillusionment, faced with the systematic use of the hypothetical-deductive method in the human sciences. On the one hand, Wittgenstein recognizes the heuristic beneficial effects of this method and, on the other, he lamented that the triumph of this method led us to say nothing of the spiritual sources that he considers as procedures just as fecund as knowledge. According to him, philosophy should buckle down to working more energetically on this task of rehabilitation of these cognitive sources. Thus, he considers that the Ancients, who were well aware of the limits of knowledge of the world, were wiser than the contemporaries who believe in a causal explanation of all social phenomena. This epistemological differentiation may be explained by an evolution in the relationship to the world and not to science. This inclusion of science in the general system of human relationships is certainly not new, but it presents the advantage of clearly showing the influence of human contingencies in the objectivist rationality of science, and at the same time emphasizing the social issues that inform it. He observes that Freudian confusion between causes and reasons, i.e. between the elements in accusation and individual motivations, in psychoanalytical experiments led him to consider his discipline as a positive discipline. However, Wittgenstein thinks that to describe a human fact is to understand it, not the subjective understanding of Dilthey, but an objective understanding, i.e. an ability to participate in a form of life.

The contestation of this scientific hijacking of the social sciences revolves around the scientific status of procedures that they adopt in their discovery approach. The polemic on the scientific nature of the social sciences crystallized with Karl Popper in the establishment of an identity between “refutability” and the scientific nature – a correspondence which condemns the social sciences to a true contradiction: to accept the disqualification of “interpretation” in social sciences or only recognize it on behalf of phenomenology and intuition of essences. Protesting against such a domination of Popper’s criteria of the scientific nature on the practice of social sciences, Jean-Claude Passeron stresses the dangers of experimental illusion (Thom 1985) and the nomological dream.

Let’s dare to give an extended metaphor to say without superfluous precautions of language: we definitely wanted to encourage epistemological thought to not shut itself in the idyllic sheepfold of quasi-experimentalism where too many *Popperoid* sheep are grazing, without ever daring to lift their eyes to the fence of their cozy park. But this is surely not to invite the emancipated sociologist to go howl with the wolves of hermeneutic usage, always ready to chomp with much relish on all scientific nature that is a little bit fragile, especially if it is rather young.

Behind this radical criticism of the perspective proposed by Karl Popper hides a measured interrogation on the conditions of production of scientific discourse and especially on its appropriateness with the uniqueness of social phenomena.

Jean-Claude Passeron reminds us again, following in the steps of Max Weber, that this debate leads us to immediately add a critique to the sociological response which establishes a relation of causality between the “appropriate meaning” which allows for interpretation of social processes through a “causal appropriateness,” and which avoids being only hermeneutic. We are less surprised then when Jean-Claude Passeron asserts the unity in the practice of the social sciences of conceptual interpretation and empirical reference and reaches the conclusion of the existence of a scientific nature particular to types of knowledge coming from “empirical sciences of interpretation.” He accepts, nevertheless, that

in an empirical science, we can qualify as interpretative all reformulation of the meaning of a relationship between descriptive concepts which, in order to transform this meaning (enrich it, displace it or simplify it), should bring in the comparison of this relationship with empirical descriptions which do not exactly suppose the very ‘universe of discourse’ as the relationship thus interpreted (Passeron 1991:401).

The relationship between the order of logic – that of concepts, and the system of facts, of empiricism – appears as a central element. If the conceptual fragility of the social sciences is in their inability to produce a “protoscholarized” language

with the virtues of a sustainable paradigm, this lack of stability is probably another no less legitimate way to induct the perceptible world. Prigogine and Stengers (1986:14) note, "Henceforth, it is based on themes of stability and instability that our descriptions of the world are organized, and not on the opposition between chance and necessity." At the end of the day, the tandem interpretation/empiricism only maintains its coherence insofar as both the status of the two instances and the function of each in the statement of the relationship itself are clearly defined. Henceforth, the relational process is at the core of the theory and practice of the social sciences. We should note that this dialogue between the concept and the real is necessarily unequal, because, as Karl Popper has observed (Popper 1959:107) himself, "theory dominates experimental work from the initial conception up until the last laboratory manipulations." Such an epistemological reconfiguration gives a particular status to the theory which inserts empiricism completely in the discovery process subjected to its control. Perhaps it would be good to note at that juncture that, against a superficial theoretical academism, science, as an "incessant polemical act of reason", is developed by following the three axioms defined by Canguilhem (Canguilhem 1957:3-12) who, borrowing the epistemological positions of Gaston Bachelard, affirms: 1) "There is no obvious first truth. There are only obvious first mistakes." 2) A speculative depreciation of intuition: "In all circumstances, the immediate should give way to the constructed"; 3) The position of the subject as a perspective of ideas: "Our thought goes towards the real; it does not depart from it."

The preconception of the series of seminars proposed over these years was to consider that the practice of social sciences in Africa was profoundly and permanently modified by consultancy which, by inscribing research in a register of voluntarist transformation, thereby transformed the rules of objectivization, observation and comparison, all of which are at the basis of the production of scientific discourse. And there is more. The cycle of urgency in which research and analysis on the social sciences is embarked reformulates fairly radically these objectives on the individual and collective levels. Thus, both processes are standardized: the conduct of the researcher and the forms of writing which normally should reproduce the complexity of the social world and the indispensable precautions which accompany the process of discovery and the tools used on this occasion. By accompanying the critic who attempts to breathe a new dynamic into the science represented by the "hard" sciences, the current metamorphosis should win over the social sciences and organize in them an original relationship with the African social world, wrongly considered as too simple and, therefore, naturally accessible by rudimentary tools. It is probably leaning on this simplistic vision that practitioners of development – the principal backers of social research – have succeeded in imposing a certain vulgar empiricism in the research method, not at all fruitful, even sterile, and analyses of an obtuse and

just as ineffective utilitarianism. This ascent of an everyday pragmatism, according to the term used by Nkolo Foé, comes from a general transformation in social relationships imposed by the logic of the capitalist dynamic. Pragmatism, according to Nkolo Foé, as a theory of knowledge dominating the field of social research is, in fact, inspired by acquired knowledge of the capitalist industrial revolution of 18th-century Europe; it claims to understand the real from empiricism or immediate experiences of objects without effort of distance. This new post-modern epistemological form, which established itself as an alternative to the Enlightenment with its universal ambitions, appears much more as a technique of social engineering searching for solutions favorable to the interests of dominant groups in crisis. Jean-Bernard Ouédraogo and Pierre Bouda's text also emphasizes this broad tendency of utilitarianism in the practice of social sciences in an Africa under the influence of developmentalists. These two authors examine the dominant current of *social engineering* and the influence that it exercises on methodology and the aim of research in social science; they compare the ideal of the researcher as a "hunter of myths" to the current figure of the practitioner of sponsored research based on a methodological fixism in the service of an overused vision of tendencies of the African social dynamic.

This form of knowledge has had more and more success in the field of the human sciences, where it inspired one Paul Feyerabend, proclaiming that "Anything goes" (Feyerabend 1979), and a certain anthropological trend claiming to conduct research without hypotheses, the law of the "field" being their only guide. This methodological process which the author qualifies as the "laboratory spirit" has basically practical and utilitarian objectives in resorting to experimental action as paths of access to knowledge and to the preparation of economic revenues. After a reconsideration of the skepticism¹ of Wittgenstein, rejecting explanation and proposals in favor of clarification, and the nominalism of Berkeley, asserting the uniqueness of things and the names that men attribute to them in classifications of appearances, the author finally finds a solution to the epistemological aporiae which characterize current philosophical debates in the philosophical dialectic of Engels, Marx and Lenin. It is true that the inscription of philosophical reason in history, to borrow the words of Hegel, allows us to reduce tensions between the individual and society and between the past, present and future. This proposal appears relevant except that this long voyage can prove to be perilous and uncertain, the relationships of man to things and knowledge having greatly evolved and perhaps been resistant to the epistemological form which Nkolo Foé proposes.

The use of theory in such a context becomes totally illegitimate and outdated. "They will only see," remarks Bachelard in a similar case, "a museum of thoughts which have become inactive, or at least thoughts that can no longer be of any worth other than as a pretext for instructional reform" (Bachelard 1971:197). A

negation of the topicality of theoretical bases which attempts to posit the social sciences as “sciences without ancestors” (Bachelard) would also like to renounce all logical filiation which binds concepts and techniques in a same history of search for the hidden meanings of the human world which is, we should recall, the classical definition of science. As Gaston Bachelard has pointed out, the attachment to scientific progress is a pedagogical element indispensable to the formation of scientific culture and serves as a framework of expression of epistemological obstacles and epistemological acts, understood, he says as “jolts of the scientific genius.” We should not see in this obvious will to renew the heuristic function of theory, a disguised way of bringing about a nostalgic return to the past and of refusing all evolution of the ways and rules of the practice of science; this recognition of theoretical filiation, according to Bachelard, “is done instead to help us become aware of the force of certain roadblocks that the past of scientific thought has formed against irrationalism” (Bachelard 1971:200). Indeed, the rejection of a certain terrorism exercised by theoreticians and sterile pedantry should not lead us to renounce concepts, categories which are fundamentally attached to scientific practice. The revitalization of theory does not at all mean a greater detachment from the empirical; the facts, as they say, are the facts. They are captured from the perceptible world and recomposed in a body of theoretical hypotheses based on conceptual fixations of practices from the past. The methodological consciousness currently underway in the social sciences stresses the indispensable liberation of the researcher from “fumbling empiricism” to accede to “the age of rigor” (Schwartz 1993); indeed, the benefit of the critique of empiricism is that it leads us to recognize that what we believed to discover in the tyrannical regime of facts is only that of which we are, ourselves, the architects. Jean-Ferdinand Mbah discusses at length the process of construction of the subject to aptly indicate the central role of concepts in this measured reconstruction of the real. As a reminder of the tidy formula which has since become a platitude of epistemological thought which affirms that the real never has the initiative since it can only respond if we interrogate it, a number of classics of social sciences would fall in line with Max Weber to stress that “these are not real relationships between ‘things’ which make up the principle of the delimitation of various scientific fields, but the conceptual relationships between problems. It is only where we apply a new method to new problems and where we then discover new perspectives that a ‘new’ science is born” (Weber 1992:146). Conceptual investment serves the sole objective of better sketching out the most secret boundaries of the social world which reveals itself at the end of a series of operations of construction, of observation and a comparative litmus test of facts. “The work of science,” writes G.G. Granger:

is thus both to formulate these configurations, to construct them, and to *think* them, i.e. to situate them, put them in perspective in a broader experience,

because science thinks. What is this talent science has for thinking? We will start our train of thought by this question, because to think means here, basically, although in various ways, *to compare knowledge to the real* (2001:9-10).

Noting the necessary break with the clear, the immediate and the real life of common sense, Jean-Ferdinand Mbah invites the researcher to go beyond the immediate subject. Emile Durkheim, very early on, confronted this methodological problem of the conceptual recomposition of social relationships and faced the reluctance that this perspective of objectivization of facts created in certain scholarly milieus; he explains the intentions in his famous preface to the second edition of *Rules of Sociological Method*.

The thing is, he stresses,

any object of knowledge which is not naturally penetrable by intelligence, all that we cannot make an appropriate concept by a simple process of mental analysis, all that the mind can only understand provided that it goes outside of itself, by means of observations and experimentation, by passing progressively from the most external characters and the most immediately accessible to the least visible and deepest. To treat facts of a certain order as things, is not then to classify them in such or such category of the real; it is to observe a certain mental attitude with respect to them. It is in addressing the study by establishing the principle that we are absolutely unaware of what they are, and that their characteristic properties, like the unknown causes on which they depend, cannot be discovered by even the most attentive introspection (Durkheim 2004:xii-xiii).

And by this recognition of the duality of the social, the researcher undertakes a conquest of the hidden meaning of social relationships which never immediately give themselves up to the knowledge of the observer. He emphasizes the major stages and the precautions to take to monitor the approach which leads to the construction of the subject in contrast with the social uses that it is used for in the common world. In a sense, the social use of the concept of tribalism in Africa is a good example to reveal the social issues, spontaneous and scholarly pre-constructions, against which the researcher should battle to conquer his subject reformulated by the articulation of hypotheses.

To consider that science is based on the combination of logical reasoning and facts is justifiable. Yet, although it is very often evoked, logic, the science of reasoning, remains suspect of evading social reality because it applies specious and abstruse reasoning to it. Whereas logic is closely related to physics and mathematics, it remains little used in the social sciences. The text that Gbocho Akissi proposes shows the methodological potentialities of various fields of logic. By examining successively the “operations of the mind” contained in language

and logical reasoning, Gbocho Akissi clearly indicates that the concepts of “reasoning,” “argument” and “demonstration” are useful to the spirit of discovery. The various stages of the construction of scientific knowledge can, therefore, be supported by the rules of logic which are not reducible to the syllogism.² The author determines clearly, with the help of concrete examples, what practicing logic means and provides the necessary tools to get around the sophist traps which normally blur the argumentation of the researcher using natural language. The links that Gbocho Akissi establishes throughout the text between logic, argument and demonstration allow him to show the utility of this “science of the combination of clauses by reasoning” in the process of discovery and in the various stages of exhibition of scientific results. Entrance into the universe of logic leads us to the recognition of foundations of the production of science, expression of the curious human mind, and thus underlines that it consists above all in representing experience in symbols. These representations and perceptible experience which these symbols refer to are then subjected to rigorous operations of demonstrative reasoning. The introduction to this exercise of logic is also an invitation to the practice of an intellectual asceticism to give priority to a clear and rigorous presentation of scientific thought. Social science research has often undergone the negative influence of certain literary tendencies which favor good expression and style to the detriment of good thought; given how much esthetic concern confines to an exclusively ethical demarcation, we would be entitled to apply to this “literary” current the somewhat severe judgment, we admit, that Jacques Bouveresse puts forward on writings on ethics: “they are reduced,” he explains, “in a certain way to conjuring the absence of the subject by the indefinite proliferation of discourse; but at no time are they able to provide the assurance that a real question was posed and that something was really said” (Bouveresse 1973:9-10).

The universe of these pre-constructions also includes science itself by imposing prescriptions on it; it is illusory to proclaim the neutrality of science when we know that it is constantly streaked with social issues among which are those which appear on a daily basis in the relationships between communities, groups, nations and races. As a result, the question of where sociological knowledge is produced, otherwise the environment in which its technical and theoretical instruments of investigation are used, becomes crucial in Africa, given that the force of the social dynamic imposes a constant and decisive renewal of the hierarchy of social values. Inherent to the case of social science's sustainability on the national level, the usual issues related to the role of social competition, which is fairly well organized, become more pronounced as it happened under traditional colonial control which established new lines of otherness corresponding to a social division between subjects and masters, and between observers and observees. Political subjects immediately become the objects of a scholarly investigation often sponsored by

the colonial power as a means of management of colonial society. This type of knowledge produced under the colonial administration and designed for its policy can rightly be considered as a science of government; one of the essential characteristics of the original approach of anthropology, as a political science, is its being a science for command and in this role to conceive of knowledge as an operation of capture and manipulation. The political meaning and the epistemological consequences of such an observation require us not to question the observer and his heuristic instruments, but rather to assess the points of prehension that he leaves on his subject of study and in so doing to locate the forgotten dimensions of this subject to commit ourselves towards its recognition. The question not only arises after the social awakening of the native who becomes a sociologist, but also follows the observation of the limits of assumptions of "objectivization," which are only transcended by a break from the dominant and normative heteronymous model permanently marked by colonial history and the encounter of two social forms. The question of the existence of this line of otherness remains well after the arrival of independence. The status of the Other is blurred, in the Moroccan case studied by A. El-Maliki, in this profound colonial history which creates an aggregate of problems and makes the construction of a new epistemology in accordance with the current social dynamic which is acutely felt. The question of the weight of colonial history in the development of social sciences in Africa comes only after the social awakening of the emancipated native; it is also the result of "objectivization" which will only be overtaken by a break with the dominant and normative heteronymous. Perhaps this subversion, by the epistemological deployment of the subject, until that point, the unspecified and passive "thing" of manipulation will help to better understand this complex world feeding our curiosity.

Even if the presence of theories were legitimate in the research process, it is not always clear to immediately agree on the conceptual types to use and the modalities of their link with methodology. Because conceptualization is not a sufficient guarantee to protect the researcher from a series of abuses linked to inaccuracy, cheating, and ideological exploitation. A critique of the idea of concept and of its roles in the apprehension of reality proves to be salutary. If theory participates in the construction of the subject by an embellished re-translation of the "problems," the very definition of social space and the sociological contradictions that it contains is never a neutral operation, sheltered from influences coming from struggles on various social issues. It is for this reason that it is indispensable to the initiation of the research process that the appropriateness of used terms and concepts be clearly assessed, and that the principal of their adjustment be posited throughout the itinerary of discovery. These theoretical constructions do not really play a heuristic role of discovery in scientific practice, as they are firmly extended towards the realization of a "strong objectivity"

towards the research of a harmony of the theory with reality. To this end, we should know how to convert abstract architectures into absolutely scientific concrete questions.

Having posited this, in order to move forward, we should recognize that the perception, though theoretical and abstract, is the result of a social struggle for the definition of the organization of things. As everyone knows now, the viewpoint creates the subject. Masked by a false neutrality of techniques and theories, involvement in the knowledge of the social world is weighed down by a series of first concepts, of “preconcepts” that Emile Durkheim checked off from the first steps of the sociological science, and which will later lead Gaston Bachelard to stress the importance of the concept of epistemological rupture which begins with the first intentions of investigation under the critical procedure of the “preliminary extrication from all involvement.” The development of this epistemological vigilance, which should be constant, is one of the most essential conditions for training in social science research.

Bringing us back to the African context, Roseline Achieng' poses the question of the local production of categories of knowledge and the relationships that this type of knowledge entertains with the academic universe, here represented by the figure of the Western scholar, and of the dilemma which appears when researchers observe the society in which they were socialized. Roseline Achieng's concern is to examine the problem of the illusion of epistemological break in researchers in social sciences studying their own societies. This question arises sharply because the information which is provided by natives is done so by the distorting filter of the “*socialization process*” of actors. Researchers should then be aware of this danger of the illusion of transparency in order to “*externalize*” these conceptual tools, and therefore contextualize the analysis of the social reality in which they are also, necessarily actors.

Roseline Achieng' proposes three paths to escape from the corruption of the “mirror effect,”: 1) the trans-historical method which consists in revisiting local history to reveal the changes in the social morphology as well as the causes of these structural mutations; 2) the comparative method which includes three phases: “contextual comparison,” of social, economic political conditions of changes over time, “triangulation of methods” which enables natives to “make their realities foreign to better understand them,” or to externalize them in order for them to be intelligible, and finally the social particularism which indicates a variation of perceptions by region, generation, race, ethnicity, gender, etc.; 3) interdisciplinarity, i.e. the enrichment of perspective of approaches between disciplines like sociology, medicine, chemistry, etc. It is also true, she says, that this collaboration includes risks in its generalization, but at least it has the advantage of allowing for a diversification of perspective of research and a refinement of the axes of questioning. We should, nevertheless, point out that the mirror effects, as

obstacles to the objectivization of research in societies with an oral tradition, do not only pertain to “native” researchers, and the history of the social sciences is made up of “scholastic distortions” in their relationships with the subject. Indeed, Pierre Bourdieu stresses, “when he refrains from analyzing the ‘theoretical’ position which he adopts with respect to his subject, social conditions which make it possible, and the gap between these conditions and those which are, in principle, the practices which he analyses. Or more simply, when he forgets that, as Bachelard reminds us, ‘the world in which we think is not the world in which we live’. The ethnologist – shut inside his scholastic ethnocentricity – can perceive a difference between the two “mentalities,” two natures, two essences, like Levy-Bruhl – and others more unobtrusively after him, when he is, in fact, dealing with a difference between two socially constructed modes of construction and understanding of the world: the first which is scholastic, and which he constitutes tacitly as the norm; the second which is practical, and which he has in common with men or women who appear to be very distant from him in time and social space, and in which he is not able to recognize the mode of practical knowledge (often magical, syncretic, in short, prelogical) which is also in his most ordinary acts and experiences (jealousy, for example) of social existence.

Scholastic ethnocentricity leads to cancelling the specificity of practical logic, either by assimilating it with scholastic logic, but in a fictive and purely theoretical way (i.e. on paper and without practical consequences), or by referring it to radical Otherness, in non-existence and the non-value of the “barbaric” or the “vulgar,” which as the Kantian concept of “barbaric taste” reminds us, is nothing other than the barbaric of the interior” (Bourdieu 2003:77-78). The danger of opaqueness that this type of relationships to the subject infers leads the researcher towards the comfortable regions of “scholastic epistemocentrism” in forgetting to “return to the world of daily existence, but armed with a fairly conscious scholarly thought of himself and his limits to be able to think the practice without destroying his subject” (Bourdieu 2003:76). We need to recognize that epistemological radicalism often hides a weak knowledge of the foundations of the research process or, even more seriously, an intention of simplification and thus of exploitation of the phenomenon studied; in these two cases, methodology is reduced to being only a fetishistic idea without any heuristic objective being assigned to it. Roseline Achieng thus attempts to warn against the harmful consequences of the propensity which African researchers have to too intense an identification with the population studied – empathy which often blinds more than it enlightens the researcher, by substituting emotion for reason, feeling for moderation, and slogans for analysis. The social reality thus becomes the hostage of the feeling of ethnic or national membership. Although it is not desirable, nor even possible, to completely reduce affectivity in the scientific relationship to the subject, Norbert Elias (Elias 1993:12) made this a criterion of distinction; he notes, “What distinguishes the scientific attitude from

prescientific attitudes, thus less distant, concerns the relative proportions of the tendencies to distancing and commitment and as well as the modalities of their fusion.”

Referring to the exception, J.-L. Moreno, Roger Cornu remarks that sociology manuals generally separate training on concepts and on techniques and methods without ever stressing their interrelations. Roger Cornu emphasizes, however, that “if we look more closely, we can observe that the slightest of methods requires a whole series of theoretical questions both for the way in which it was produced and for its mediating between theory and the subject studied, or even in the way in which it is used” (Cornu 2005:394) ; and on the other side of research, he observes that “the sociological imagination exists only insofar as we consider that the question of tools is not a simple technical problem but that it implies theoretical issues”(Cornu 2005:395). Although we cannot always show the difference between theory and technique, it is always wise to construct a distinction between these two terms of the heuristic process and to clearly portray the foundations and theories which articulate their relationships. This is probably the moment to stress the importance of theory in research and to, therefore, break with the representations, as false as they are common, of theory seen as a sort of pointless speculation completely devoted to the realization of a uniquely abstract design. Yet, there is no need to come back to the modalities of the abstract (re)construction of the perceptible world with the goal of better discovering its most secret articulations. Is there a reason to suspect that theoretical formulation is incompatible with understanding social reality, because it is too far removed from it? On the contrary, is it justifiable to only stick to the practical function of techniques meant to immediately bring us closer to “reality”? Entire works on methodology in the social sciences refer recurrently to this false opposition and invariably provide a series of conventional responses to it.

Life stories, understood as a technique of investigation and discovery, correspond very well with the interrogation on the neutrality of techniques used in the social sciences. This reveals particularly the epistemological bearing of this seemingly banal operation of life itinerary collection. This technique places the problematic relationships between individual and society at the core of the debate; and this approach, by the apparent facility of its exercise, nevertheless hides a mass of obstacles which, if they are not overcome, might limit the performance of the approach. We should recall the acerbic critique (Bourdieu 1986) stirred up by the arrival of this approach to life stories which consisted in making the “subject” sacred and accepting a certain philosophy of life unfolding, following a chronology in which the subject would only be the corporal manifestation. It would then be sufficient to collect indices of this linearity by carefully following the sequential logic of the order of things and forgetting that this organization is a sort of “artifact,” an “artificial creation of the senses.” The main argument of this criti-

que consists in putting to the test the coherence of the narrative, which intends to impose a “significant sequence” and refuse an identification that presupposed the life story as the trajectory of a finished substance. And it is clear that recognition, even conventional, of the individual supposes that the influence, however decisive, of a whole social system be minimized, without which the singular element can neither exist nor assert itself as a particular subject. This is why we can understand and accept the argument of Pierre Bourdieu who attests that: “trying to understand a life as a unique, self-sufficient series of successive events without any other link than the association to a ‘subject’ whose consistency is probably only that of a proper noun, is almost as absurd as trying to make sense of a metro trip without considering the structure of the network, i.e. the matrix of objective relations between the various stations” (Bourdieu 1986:72). Following this critique of the false evidence of the unity and individual singularity, the question still remains as to the legitimacy of this approach and especially as to the way in which it could promote a better understanding of social processes which are seen in the interaction of life in society. Mokhtar El Harras, in reconstructing his Moroccan experience, stresses the complexity of the life story, the obstacles and ways to minimize them. He wonders how to overcome the fictional aspects of the life story and in what way anthropology can benefit from this particular approach.

Whereas films and photographs produced in Africa seem to confirm a certain exoticism, a sort of naïve distance for an aesthetic consumption of clichés, analysts have forgotten to question the use of the image as a means of investigation and discovery of social relationships. In her text, Clara Carvalho discusses the importance of audio-visual instruments (film, photography) in anthropological research. Referring to discussions which took place at the beginning of the 1970s, she leads us back to the origins of what will later be visual anthropology, born out of a tension between those who advocate the “anthropology of urgency” (Margaret Mead) and use film and photography as auxiliary means of research, and those partisans of an anthropology which assumes the form of a knowledge in which the relationship between the subject and the observer constitutes the foundation of a “dense” textual “description” (Kirsten Hastrup). The scientific potentialities of this new means of expression of human personal experiences have long been neglected within this discipline. The use of film and photography has long been considered a secondary epistemological act, despite the fact that it still has a number of enthusiastic defenders. Among the pioneers who begin to use film as an efficient research tool we can cite Marcel Griaule with his film “Au pays des Dogons [“Dogon Country”]” produced in 1935. Anne Attané presents concrete cases of the use of photography in the practice of social sciences and succeeds in showing that photography is perhaps not a means of construction and investigation throughout the research process, but it can be used, in still rare cases, effectively in the research results presentation phase as an autonomous

modality of the exposition of a final argument, without being pigeon-holed into the thankless and passive role of the “illustrative vignette.” In stressing the limits of the use of iconography in research, Anne Attané observes that the use of photography requires a good mastery of photographic language and recognizes that this new tool does not replace the classic means of research, but calls for combination and collaboration, thus an improvement in our practices by a reciprocal improvement in tools used.

In the early days of sociological science, the founding fathers discussed the possibility of using a comparison, of establishing causal relationships between various forms of “human development.” Émile Durkheim (2004) found in the “comparative method” the basis of the administration of proof in sociology. More recently, Marcel Étienne (2000) searched for the implementation of a “constructive comparatism,” thanks to an alliance between the historian and the anthropologist. The development in the African context of a demand for applied research coming from supranational organizations has abundantly solicited hence leading the comparatist approach astray – used with no rigor, reduced to being only a simple listing of disparate and associated facts without adequate theoretical foundations. The approach has had great success in various fields of the social sciences without any confrontation of the disciplinary specializations and the parallel development of methods being used to compare different orders. In this way, the African reader will find in this text a well-crafted presentation of the procedures and hypotheses in play from the comparatist perspective. Cécile Vigour, after having probably provided the most informed and most complete work on comparison, proposes a clear and precise synthesis of the conditions of exercise of comparison, the success of which very often expresses a great ignorance of epistemological, technical and political conditions. Comparison is not the simple proximity of facts. It refers strictly to principles and issues of the comparative approach and finds strategies to explain evident social facts in at least two entities. Cécile Vigour suggests that epistemological and methodological thought should be conducted prior to any comparatist approach. This leads necessarily to a general interrogation on the production process in the social sciences. In this way, comparison leads us to revisit the foundations of the social sciences.

Paradoxically, writing appears as a dismissal of research work. Often considered a prerequisite, an ordinary given of all intellectual work, it seems to be taken for granted, and its more or less brilliant mastery adds to the dexterity of academic competence. We forget, however, that for certain literary figures like Flaubert (De Biasi 1995), “to think well is to write well” is a good definition of literary work. This subordination of writing to thought is based on the heuristic function of language and as a result, is, according to Popper, consubstantial with the formation of the critical method which is the foundation of scientific progress. Indeed, he notes that, “The critical method even presupposes writing as much as

possible” (Popper 1997:21). Bidet and Lemeur’s text invites us to be vigilant of the false neutrality of writing and the abusive use of the effect of “pathos” in the exercise of research work; they then revisit the process of knowledge production through stressing the active function of discovery of writing and reading in each sentence. The two closely linked activities feed on each other as the researcher writes texts that he rereads and gives others to read just as he gains in knowledge by reading others. The two authors show the pressing need of being wary with respect to “artificial paradises of formalism” in writing, effects of styles which often make us forget that in all research work, as Bolzano suggests, it is necessary “to say clearly what we are talking about, in what way we are using such or such word, and then to indicate for what reasons we are asserting such or such a thing, etc.” (cited in Bouveresse 1999). After reading this text, it seems clear that it is not in the extreme stylization of scientific language that the social sciences will impose “their epistemological status by tearing them away from the sins of natural language” (Passeron 1991:154). Bidet and Lemeur’s text stresses the fact that writing in scientific practices is not only a modality of expression, a style, but is also a mode of knowledge and of discovery.

The texts included in this volume have the sole objective of enabling readers to consider with critical distance scientific commitment which suffers from a sterile utilitarianism often corrupting the creativity of researchers in the African context. It would seem that we should now suggest a break with the dominant *doxa* which, supported in this undertaking by developmentalists, refused to allow African researchers free rein in abstraction and requires them to occupy the thankless place of data purveyors in a global division of intellectual work where the spot favorable to the accumulation of symbolic benefits of recognition is forbidden to them. How can we construct a science, monitor its practices and formulate theories on which it should thrive without mastering the epistemological prerequisites and thereby daring to offer a measured contextualization of its results? This volume is an incentive for a critical look back on social science as it is practiced in contemporary Africa. The intention of the authors of this volume will have been diverted if after the reading of these texts it does not clearly appear that “methodology is not the private tutor or guardian of the scholar, but always his student.”

Notes

1. We will agree on the skepticism of Wittgenstein and his great distrust with respect to psychoanalysis and the anthropology of Frazer and accept along with Jacques Bouveresse that, “the phenomenon which seems to have most attracted Wittgenstein’s attention at a certain time is that of the transmutation of an interesting hypothesis into an *a priori* truth from the clarifying point of view into a mode of obsessive representation, from a revolutionary formula into a consecrated formula, from a theory into a myth.” in *Wittgenstein: La rime et la raison. Science, éthique et esthétique [Rhyme and Reason. Science, Ethics and Esthetics]*, Minuit, 1973, p. 27.
2. The obligatory and incontestable profession of this type of reasoning which deals with objects as an entity without “quality,” constructed from an extreme creates mistrust and suspicion.

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PART ONE

SOCIAL REASONS FOR
SCIENTIFIC PRACTICE

1

Pragmatism as a Vision of the World and as a Method: A Philosophical Examination of the Challenges Presented to Contemporary Social Research by Subjective Idealism

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Carried along by the current wave of postmodernism, today, pragmatism dominates the entire social, economic and cultural field. With respect to epistemology, its world vision and its methodology have succeeded in establishing themselves in all sectors of research in the sciences of man and society: philosophy, literature, sociology, economics, political science, etc. As a theory of knowledge, pragmatism makes the claim of understanding reality starting from views of “radical empiricism.” Thus, it merges reality with “experience,” i.e., with the satisfaction of subjective interests of the informed subject. It is here then that pragmatism meets the central problem of subjective idealism, which relates man’s knowledge of the world to the content of his own consciousness. The decisive question raised then by subjective idealism and pragmatism is the following: is knowledge of the objective world possible? Essential in epistemology and in methodology, this question involves another, that of the very possibility of objective truth and absolute truth in the undertaking of knowledge. This article will attempt to answer these various questions.

The Historical Context of Pragmatism

The current trend of pragmatism and subjective idealism is inseparable from the global expansion of capitalism. At the same time as its development at the end of the 19th century, this doctrine quickly emerged as the philosophy of the advanced

industrial bourgeoisie. Its objective was to offer a credible alternative to the Enlightenment of the previous century. A historical reminder would be useful here. The philosophy of the Enlightenment, with its concepts of Reason, the Universal and Freedom, established itself on modern consciousness as a world view of a bourgeoisie in search of its own identity. This was a century *par excellence* of the reform of mentalities and social and political revolution; 18th century Europe needed a coherent and stable body of principles. The cosmic and historical odyssey of reason, such as it was later synthesized by Hegel, encapsulates this vision of the world. By clear and unequivocal affirmation of the principle of reason and the universal, the new bourgeois Europe, recently emerged from the Middle Ages, provided itself with theoretical, cultural and political means to realize its essential historical purpose, thanks to science and technology, but also to the construction of a modern capitalist economy and a liberal and democratic state. As defined by the thinkers of the Enlightenment, and also by Hegel himself, the idea of reason and the universal certainly appears as a global vision of the world, but also as general explicative principle of the enigmas of the universe.

The formation of pragmatism and the rediscovery of subjective idealism (inspired both by ancient sophistry, the sensualism of George Berkeley and the empirio-criticism of Ernst Mach) attempted to provide a philosophical response to the decline of the great systems directed towards a global explanation of the world, whether it be the Enlightenment itself or Hegelianism or Marxism. The particular character of these systems was to adopt a project or provide a utopia, a vision. In advanced capitalist societies, it was hedonism – as a moral and social ideal – and the cult of the moment, which replaced vision, utopia, the meaning of history and perspectives. Corresponding to the triumph of positivism, it was this period which saw philosophy renounce its deepest-rooted claim which, since Descartes, had been to transform the world. Henceforth, philosophy could allow itself to downwardly revise its ambitions by assigning itself a minimum task, i.e., to interpret the world. By way of example: Wittgenstein established that as it is not a doctrine, but a mere activity, the only true goal of philosophy consisted in the logical clarification of thought. If, for Wittgenstein, a philosophical work consists essentially of clarifications and not explanations or suggestions, it is because “the objective of philosophy is to clarify and rigorously define the scope of thoughts which otherwise are, as it were, confused and blurred” (Wittgenstein 1961:52). Analytical philosophy gave these views their radical form. In his critique of this latter current of thought, Herbert Marcuse sized up these questions, by taking up the challenge raised by systems of this sort, not only in philosophy and methodology, but also in social thought itself, in its totality.

Let us return to the subject of pragmatism. Whether it be Charles S. Peirce, John Dewey or even William James, pragmatism refuses to look at reason and science as a global explanation of the world. Far from being an attempt to decipher

enigmas of the universe, these enigmas appear from this point on as a simple way to act on the real. As theories particular to the advanced industrial era, pragmatism and subjective idealism mean that – in the same way as empiricism, nominalism and utilitarianism with which they are joined – after the great revolution of the 18th century, bourgeois society no longer had any plans to formulate on a global society: no radical metaphysics to promote, nor any theoretical explanation of the world to provide. From that point of view, society can content itself with *pragmatically* managing the acquired knowledge of the industrial revolution, by settling for promoting the *laboratory spirit*, in the exclusive service of the capitalist economy.

Laboratory Spirit, Methodology and Theory of Knowledge

To inaugurate a new era of Enlightenment, such was the ambition of pragmatism. Unlike the Enlightenment of the 18th century which incarnated with the ideals of reason, the universal and freedom, the new Enlightenment brought on by the laboratory spirit would mean the clarification of ideas with the help of experiments. Skeptical with respect to the “natural light of reason,” the horizon of pragmatism was limited to light constructed artificially in the experiment. Pragmatism can, thereby, be seen as the incarnation *par excellence* of the laboratory spirit. The characteristic trait of this spirit was to think of all things as in a laboratory, i.e., in the sense of experimentation. If the experiment is the guaranteed starting point of thought, it is because it appears as the privileged instrument in the hands of the researcher to avoid errors and arrive at a certain knowledge. In short, the issue is to submit all concepts by which the researcher operates to experiment so that it can reveal to him what these concepts intrinsically conceal. The concept of force is sufficient to illustrate since it only refers to the sum of its effects. It is thus only through their consequences that we are capable of recognizing objects and it is in the experiment that these consequences reveal themselves with the greatest clarity.

This approach had direct consequences on the very concept of truth. The laboratory spirit meant that, from then on, neither the inspired knowledge of priests, nor the subtle logic of metaphysicists, nor the scholarly dialectic of philosophers, is any longer the source of truth. As a methodology, pragmatism teaches that the researcher who engages in experimentation is not meant to start from an *a priori* idea. He should, rather, attempt to directly confront reality in order to force it to reveal its mysteries to him. We can, therefore, better understand the concept of *practical* which refers narrowly to the experimental action to which each idea should be subjected to be legitimized. It is the Greek etymology itself, *pragma*, i.e. *action*, which inspired the theoreticians of pragmatism. Claiming that our beliefs only make up rules for action, pragmatism asserts that to develop the content of an idea, the researcher has only to determine the conduct that this idea

is likely to provoke (James 1968:49). This means that in order that thoughts concerning a given object might be clear, it is above all important to identify which practical effects the object in question contains. Pragmatism, therefore, postulates the absolute primacy of the act on all which is thought. A conclusion is called for at this juncture: the validity of reasoning is, principally, a question of fact and not of idea or thought.

It is on this precise point that the pragmatist approach was able to establish a radical opposition between the spirit of modern science and the spirit of ancient metaphysics. The pragmatist method sees ancient metaphysics as an attempt at sublimation of the self-contained world, made up of a limited number of fixed forms on the inside and set off by rigid boundaries on the outside. On the contrary, pragmatism sees in the spirit of modern science, a world open *ad infinitum* and varied. This is, in short, a world without limits, stretching beyond all definite markers. The spirit of modern science then corresponds to the ruin not only of immutable substance, but also of the idea of truth and certainty referred to fixed objects with fixed properties. When J. Dewey attacks the question of certainty, he stresses that research on the subject is confused with research on experimental methods of control, i.e. regulation of conditions of change compared with their consequences. Therefore, the search for certainty is assimilated into the quest for practical certainty, security and the safety of instrumental operations. Dewey sees in scientific objects simple control instrumentalities. The control instrumentalities should be understood as objects of reality itself and not as discoveries of the immanent properties of real substances (Marcuse 1967:77).

It seems that here we have one of the most radical critiques of “ontological metaphysics” in the permanent quest for essences. Once the basic postulates of such a metaphysics have been removed, philosophy and science are henceforth reduced to a series of questions that the researcher can finally submit to observation, in the definition of exact science. C. S. Peirce, for example, could only grant some interest to philosophy insofar as it was likely to reduce fundamental philosophical questions to simple scientific questions. In its attempt to forcibly bring philosophy into the Procrustean bed of science, pragmatism, thereby, betrays its well-known cohabitation with the most radical trends in modern positivism.

Let us take the question of the validity test. With experimental science, this question undergoes a radical transformation, compared to the approach coming out of Newtonian physics, for example. In this approach, it is the inherent properties of real objects that are mainly targeted in the validity test, ones isolated from the others, but firmly fixed and immutable. In contrast, with respect to experimental inquiry, the validity of the subjects of thought mainly depends on the consequences defining these subjects. Contrary then to Newton’s mechanism which postulates the unity and immutability of the world order, the experimental spirit removes from the world not only its unity and order, but also its stability

and eternity. It is the same experimental spirit which permanently places pragmatism in sensualism, thereby justifying its hostility with respect to abstraction as a principle of knowledge. In turning away from abstraction, this doctrine claims to turn away “from anything that makes thought inadequate [...], from anything so-called absolute or an alleged origin, to turn towards concrete and appropriate thought, towards facts, towards effective action” (James 1968:52). It is in this way that belief founded on the authority of reason and abstractions was replaced by belief in the unpredictable revelations of the ever new and open experiment. The ancient universe founded on formal abstractions such as “God,” “Matter,” “Reason,” the “Absolute,” could only retreat faced with the immensity and the kaleidoscopic stream of phenomena.

These sensualist views are not peculiar to pragmatism but can be traced back at least to Berkeley (1944). His *prosopopee* on Philonous and Hylas is interesting from this standpoint. Rebelling resolutely against traditional metaphysics, the Anglican bishop was already worried about the fact that our knowledge of facts has been led astray by the false hypothesis of the double existence of perceptible things. Berkeley believed neither in the existence of thought, nor that of the world. In fact, the existence of matter and its reflection in thought is called into question. Such is the issue in the dialogue between the Friend of the Mind (Philonous) and the Friend of Matter (Hylas).

As his name indicates, Hylas is a materialist. For him, the perceptible appearances of things, colors, forms, etc. provide information on the way in which phenomena appear to the consciousness that perceives. Essences exist behind these phenomena. On the other hand, Philonous incarnates immaterialism. As a sensualist, he states that things do not exist independently of the sensations of the informed subject. The phenomena are only a complex of sensations, a sum of mental representations or a group of ideas, and not the reflection of the external world. Both the form and the area that these phenomena occupy in space constitute sensations. The yellow color of the orange is only a visual sensation, the contact of my hand, a tactile sensation, the flavor that I taste, a mere state of consciousness. According to Philonous, things only have reality to the extent that they are perceived, touched, tasted, felt. Consequently, I cannot really allow myself to state that an idea of the thing exists, or that the thing is reflected in my consciousness. The thing is simply a set of ideas and nothing more. If Hylas admits the existence of a material substance, an essence hidden behind perceptible appearances, Philonous, on the other hand, denies the existence of such a substance beneath the perceptible. By so doing, he transforms consciousness or the mind into a demiurge, since Philonous wants to transform things into ideas, pure representations of ideas in things.

As a concrete realist, Berkeley does not understand that Descartes dares to doubt the senses, whereas they constitute, in his mind, the true seat of phenomena. Berkeley is convinced that the world which unfolds in front of us is really colored,

sonorous, soft or hard, as it appears to us. That is to say that the perceptible representations, i.e., ideas, make up reality itself: the appearance and the phenomenon are the very being of the world. Hence, *phenomenology* – i.e. the description that we make of the world – and *ontology* coincide; they are not in opposition as in the philosophy of Plato, for example.

In this philosophy, the only things that truly exist are those that we can perceive with our senses. As a result, all which escapes our perception does not exist. This means that matter, according to Berkeley, is coextensive with our representations. And this matter has neither substance, nor essence; it does not refer to an unknowable in-itself, i.e. things in the world are transparent, spread out as they are on the surface. Essence and substance, the in-itself and the ontological substrate of phenomena only appear as metaphysical fictions to Berkeley. The kaleidoscopic flux of phenomena are the only things which truly exist, changing and diverse appearances. Thus, for example, the fruit that I see as round and yellow, and the one that I touch and perceive as smooth or rough, do not actually refer to the same object. Because the real fruit that exists in nature that might be both round and yellow, rough and smooth. What really exist are diverse, simultaneous or successive appearances. The unity of the supposed thing is, according to Berkeley, only the unity of the name under which men regroup some appearances. Such is the foundation of nominalism. Such a nominalism means that the thing only derives its unity from its deepest essence, and thus, far from being real or substantial, this unit refers simply to a convention.

The Question of Objective Truth

Two decisive questions are hidden behind these sensualist views: the question of the existence of the objective world and that of objective and absolute truth. Pragmatism rejects the claim of man to arrive at objective and absolute truth. This contestation is justified because of the impossibility that man could faithfully represent the external world. Any attempt to represent such a world is doomed to failure. A neo-pragmatist like Richard Rorty is truly convinced that human consciousness – and, therefore, the philosophy which is a witness to it – is not the “mirror of nature,” and that contrary to their claims, no science is capable of reflecting the true essence of things and being. Hence, the title of his work, *Philosophy and the Mirror of Nature*, where he strikes out at any idea of reflection.

Against the theory of reflection which postulates the objective knowledge of laws of nature and a certain approach to the absolute, Rorty reintroduces outdated concepts particular to empirio-criticism, for example, consensus, convention, description, justification, and convenience, etc.

Let us examine the case of consensus. Pragmatism is defined as anti-essentialism. As such, it does not define the “objective” according to a relationship with the essence of things, but simply “according to the ease with which those who ob-

serve these things arrive at a consensus” (Rorty 1995:64). It is the “degree of ease with which the subjects create a consensus” that pragmatism substitutes for the former objective-subjective distinction. Thus, to assert, for example, that values are more subjective than facts, is simply to say that it is less easy to agree on the beautiful, the ugly, good and evil than on geometric figures, for example.

Let us take another example, justification or more precisely, justified belief. By placing justified belief at the center of any process of knowledge, pragmatism always aims to delegitimize objective and absolute truth. “Truth is what is meant to distinguish knowledge from well-founded opinion, from justified belief,” writes R. Rorty. But, he adds, “if truth is, as James said, “the name that we give to everything that proves to be favorable to belief... we can obviously not see how truth would differ from what is justified” (Rorty 1995:33). Rorty’s conviction is that there is absolutely nothing to say on the subject of truth, and that philosophy should limit itself to justification or to guaranteed assertibility. The reason for this is that the concept of truth does not seem to be of greater use than the idea of “correspondence with the real.” Rorty is convinced that if “a way of being in the world” scarcely exists, or that “nothing such as the intrinsic nature of reality exists,” that means that neither does there exist a way in which this reality should be represented. What does exist, on the other hand, is “causal pressure” (Rorty 1995:36) or the multiple “ways of acting in order to achieve human hopes for happiness.” Yet, considering that access to such happiness does not generally differ from justified belief, it is thus legitimate to abandon any idea of representation of reality.

Such an approach to knowledge and truth makes any idea of certainty illusory. To speak of the search for certainty is, for pragmatists, an attempt to flee from the real. The researcher should, therefore, abandon the concern of knowing if what he believes is well-founded or not, and should instead ask if he possesses sufficient imagination to develop interesting alternatives to his own beliefs (Rorty 1995:37).

Substituting hope for knowledge, as Rorty does, takes on a particular interest for pragmatism. This doctrine is concerned with definitively eliminating fundamental concepts of modern philosophy, in relation, for example, with nature and the limits of human knowledge, the epistemic situation of man, etc. The things being thus considered, the researcher will learn to renounce all attempts at knowledge of the external world which, according to the classical theory of knowledge, would begin, for example, by the “data of the senses.” The researcher should renounce the idea of a “natural order of reasons” to which each person should conform to justify his own beliefs, because, as perspicacious as he may be, he has no means by which to distinguish between science and non-science. Thus, considering the absurdity of a “natural order of reasons” to which each person should conform to justify his beliefs, it is, as a result, necessary to admit the legitimacy of all beliefs. For example, science and religion should both be

considered as two legitimate paths also leading to true beliefs, although these beliefs respond to completely different ends.

Thus, as a theory of knowledge and as methodology, pragmatism rejects truth as an epistemic truth concept. In fact, for this doctrine, truth is not the objective of research, since research itself and justification are capable of pursuing a multitude of particular goals. And, furthermore, there is not a superior objective, which, as a result, would dominate all the other goals and which would be called "truth."

The Privilege of Language and Description

Against the concept of absolute, objective truth, Rorty's neo-pragmatism asserts the infinite privilege of language to which he relates the entire consciousness. Isn't all consciousness a question of language? It is here that pragmatism encounters psychological nominalism. Concerned with the "linguistic dissolution of reality" (Morilhat 2004:107-110), such a nominalism means that the human being will never be capable of taking a step outside of language which describes phenomena. In the same fashion, he will not be capable of grasping reality outside of the mediation of linguistic description.

Once the distinction between appearance and reality suppressed, pragmatism endeavors to replace this dualism by a much more operative and realistic distinction, namely, the distinction between a less useful description of the world and a more useful description of this world (Rorty 1995:59). According to these views, the researcher does not attempt to enter into a relationship with the real in order to know it or to discover the truth of its essence. The methodological approach of pragmatism, on the contrary, calls on the researcher to be more self-effacing: he should content himself with describing the real according to his needs. And what each of us should retain from this world is not so much the truth of its essence as what is useful to us. As Rorty adds, psychological nominalism is "the corollary of the doctrine according to which there is nothing to know outside of what is affirmed in the statements which describe it" (Rorty 1995:69). It is thus because each sentence stated about an object constitutes an implicit or explicit description of a relationship that this object has with other objects. Let us take an example. All that I know about my table, for example, is that it is rectangular or square, smooth or rough, that it was made from such or such tree species of the forest, that I use it as furniture, etc. Thus, pragmatism teaches us that there is plainly nothing to know about this object, outside of truthful sentences which enable me to witness this reality. Pragmatism recognizes an exclusive role for sentences, that of establishing relationships between objects. In describing objects in the world, sentences also attribute a relational property to them. An example: let's suppose that we are trying to find out what the table is intrinsically. The best response that we can obtain is the following: it is "that what we can truthfully say is that it is brown, it's ugly, that it

hurts your fists if you hit it, that you can run into it, that it is made of atoms, etc.” (Rorty 1995:74). Pragmatism then definitively asserts the impossibility of going beyond language to reach some form of non-linguistic knowledge.

Richard Rorty saw modern physics as the ultimate refuge for researchers who still believe in the existence of a universe external to language or the consciousness. Indeed, the great illusion of physics consists in believing that it is capable of thrusting us outside of ourselves, our language, our needs or our objectives. Rorty is convinced that physics can teach us nothing about the world or even about the intrinsic nature of things. Its only quality lies in the practical utility of its descriptions of the world. As the other sciences, physics should be part of human plans.

We should note that, despite appearances, this question is not new; it is already present not only in Wittgenstein’s linguistic problem, but also in the epistemology of a physicist like H. Poincaré. Relativism and subjectivism are the common ground of these doctrines, even applied to a field such as geometry.

In fact, for Poincaré, space and time appear as purely mental constructions. Instead of the world imposing them on us, as it is currently accepted, it is we, on the contrary, who impose time and space on the world. Poincaré made two kinds of spaces coexist: a space called geometric and objective and a representative space. According to the physicist, the latter can be broken down into a tactile space, a visual space, and a driving space (Poincaré 1968:245). Poincaré’s point of view is that the researcher does not represent external bodies for himself in geometric space; he contents himself with thinking about these bodies, as if they were situated in geometric space (Poincaré 1968:82). It is in only this way that he gives a privileged place to consciousness, to the point of definitely installing geometric “law” in relativism and subjectivism. His viewpoint is that all geometries are essentially relative; none can be truer than another. The difference between one geometry and another lies simply in the fact that one geometry can be more convenient than another (Poincaré 1968:76).

The concepts of human plans, convenience, goals, needs, etc., imply not only the repudiation of objective truth but also the fact that there are an infinite number of approaches, descriptions or even points of view on a same subject. It is only in this way that pragmatism renders all quest for truth in the scientific approach vain. And, in the absolute, scientific research itself, as a requirement for truth and certainty, becomes without a subject. From the cultural standpoint, pragmatism establishes the legitimacy of all human plans. According to Rorty, no one should be allowed to ridicule any human plan or even any deliberate form of human life. This means that each person is free to consider true what another may hold to be false. Moreover, Rorty teaches us that it is perfectly useless to try to convince an interlocutor who does not share the same needs as you, because, all “discussion requires that one agree on the precedence of needs” (Rorty 1995:84). The impor-

tant point in a discussion, he writes, is that we agree on the use of the same instruments to work towards satisfying shared needs. Because, no debate, no argument, however rigorous it may be, can succeed in modifying the central plan of an individual and lead him to change his point of view. Changing point of view on a particular question means that the interlocutor no longer sees the interest or the relevance of the arguments that he was defending up until then. Because reason and truth are only myths, and because the ordinary man is far from being a cognitive being, absolutely nothing can make him likely to be converted by arguments rather than knocked over by irrational forces (Rorty 1995:86).

The pragmatist approach leads unavoidably to the sophistry. Moreover, pragmatists fully accept the contested views of Protagoras according to which man is the benchmark of all things. Everything is beyond all discussion, both scientific facts and moral values. These concepts defy all analysis because they correspond to a way of saying: "This is my position: I cannot say anything about it; I cannot do anything else" (Rorty 1995:121).

Dialectic Approach of Questions Raised by Pragmatism

The approach of the real proposed by pragmatism raises enormous methodological problems. For example, no one can seriously question the concepts of objective reality and absolute truth without sinking into the worst of difficulties. Yet, as Lenin notes with good reason, reducing the concept of truth to adjustment, belief, justification, consensus and simple convenience, is to take a collection of words for theory. So that its hypotheses might be true, pragmatism should first prove that the most indisputable scientific laws are only useful fictions or even the result of consensus between researchers. He must prove that the assertion that the earth is round, that it has a history and turns around the sun is a mere convention, a convenience, a belief; that, therefore, it is up to us to believe or not to believe this. And yet, we know that those truths are not only objective and absolute, but also eternal. Such an attitude is as absurd as claiming that the slavery of Africans and the Holocaust of the Jews are only relative truths, from the imagination of some individuals assessing the world from their own point of view. Those are some examples of indisputable absolute and eternal truths, which then depend neither on my point of view nor on my belief, and even less on simple convenience, agreement or justification.

Victims of their poor methodological choices, pragmatism and subjective idealism are not able to admit that the world, such as it exists independent of us, is reflected in our consciousness through the senses. There is no doubt that sensation is the primary source of our knowledge, as the Pharaonic theory of knowledge had discovered. According to the document of Memphite philosophy, the eyes see, the ears hear, the nose breathes, they provide information to the heart (understanding); and it is the latter which gives all knowledge and it is

language which transmits what the heart has ordered. Certainly, the theory of sensation can also lead us into subjectivism and relativism, as we can see in the work of Berkeley who considers bodies a complex of sensations. The truly scientific approach, which is based on the dialectic method, is just the opposite. It recognizes sensations as images of bodies and of the external world. This approach prepares us then to admit the existence not only of an objective world independent of our sensations and sentences that we use to describe them, but also of objective, absolute truth. Objective and absolute truth exists because objective reality itself exists.

Let us correctly situate this problem within the theory of knowledge, and examine the dialectical movement which leads to absolute, eternal truth. The main weakness of pragmatism is that, since the time of Charles Peirce, John Dewey and William James, this doctrine has never been able to correctly pose the problem of absolute truth, because we do not resolve any problems by coming out right and left with pompous and deafening expressions like: convention, convenience, adjustment, belief. The most important scientific and philosophical task is, on the contrary, to resolve dialectically the problem of close relations between absolute truth and relative truth.

In appropriate terms, F. Engels was able to grasp the issue of this question in *Anti-Dühring*, where he poses and clearly answers “the question of whether the products of human knowledge, and which ones, can have a supreme validity and an absolute right to truth” (Engels 1973:117).

To answer this decisive question, Engels recommends examining first what is human thought itself, in its profound essence. Is it the thought of an individual or that of humanity in its totality? According to Engels, far from being an individual matter, human thought deserves to be understood as the thought of humanity, taken as a whole. This thought, however, can only exist concretely “as the individual thought of billions and billions of men, past, present, and future.” (*Ibid.*) This is then how the author expresses dialectically the contradiction between the absolute character of human thought and its actualization in living beings with extremely limited thought. In fact, according to him, “the sovereignty of thought is born out in a series of men whose thought is hardly sovereign, and the strong knowledge of a right to absolute truth, in a series of relative errors. Neither one nor the other can be realized completely except by an infinite duration of the life of humanity” (Engels 1973:117).

For Engels, such a contradiction can only be resolved in infinite progress, i.e. in the unlimited succession of human generations. It is only in this sense that one can say of human thought that it is just as sovereign as non-sovereign, as absolute as non-absolute. “Sovereign and unlimited by its nature,” he adds, “its purpose, its possibilities and its final historical objective; non-sovereign and limited by its individual execution and particular reality” (Engels 1973:118). It is the same dialectic

of the relative and absolute that the thinker applies to “eternal truths.” For him, “if humanity ever came to the point of no longer operating with eternal truths, results of thought having a sovereign truth and an absolute right to the truth,” this would boil down to stating that it has arrived “at the point where the infinity of the intellectual world is depleted in deeds as in power, and thus accomplishes the much discussed feat of the counted uncountable.”

Such are the arguments that philosophical materialism uses to escape not only from dogmatism but also from relativism. It is dialectics itself that is the privileged methodological tool enabling us to reach such a conclusion. Dialectics enables us to assert the infinite power of human thought, all the while recognizing its historical relativity. It is in this sense that we can say that, objectively, there is no line of impassable demarcation between absolute truth and relative truth or even, between truth and error.

The dialectic of truth and error, of absolute and relative, distances us more and more, not only from dogmatism, but also from relativism, characteristic of all non-dialectic thought. This latter oscillates constantly between the dogmatism of absolute truth and the dogmatism of absolute negation, hence relativism. In general, relativists are, according to Henri Lefebvre’s term, “pessimists of knowledge, embittered, disillusioned by metaphysics, who miss absolute truth and state with a contained anger that this ‘noumenal’ truth exists but escapes us” (Lefebvre 1982:67). Such is, for example, the Kantian version of agnosticism and relativism. The pragmatist and postmodernist version is even more radical, since it denies the very existence of “*noumenal truth*.” As we see in the work of R. Rorty, pragmatism is an anti-essentialist doctrine, which denies the existence not only of absolute and eternal truths, but also of essences and substances. Instead of essences, this doctrine sees only moving nodes of relations.

Let us conclude on this point. Contrary to the relativism of those disillusioned by metaphysics (neo-Kantians and pragmatists combined), dialectical relativism is fundamentally optimistic. If it recognizes the relativity of knowledge, it is not because of some “metaphysical inevitability” or some infirmity of human reason condemned to never be able to penetrate the essence of things. Relativity can be explained simply “with respect to the stage actually attained by our knowledge” (Lefebvre 1982:67), i.e. dialectical relativism postulates the relativity of human knowledge, not to repudiate the concept of objective truth as such, but to emphasize the perpetual and infinite overrunning of the limits of knowledge. Dialectical relativism teaches us that each new stage of development of human knowledge enriches it with new grains of an ever broader, more specific, finer truth. It is in this way that we can state that each particular truth attained is essentially relative. However, the set of the crop of particular truths attained by human knowledge is part of a vast set of objective absolute knowledge.

Lenin clearly established this: if “the limits of the approximation of our knowledge compared to objective, absolute truth are historically relative,” there is no doubt that “the very existence of this truth is certain as it is certain that we are approaching it” (Lenin 1979:129). The analogy of the painting presented by the author is interesting in this respect. The outlines of the painting “are historically relative, but it is certain that the painting reproduces a model existing objectively.” According to this thinker, the fact that such or such moment, in such or such conditions, we have advanced in the nature of the knowledge of things to the point of discovering alizarin in coal tar or discovering electrons in the atom, is historically relative; but what is certain, is that any discovery of this sort is progress in “absolute objective knowledge.” In short, all ideology is historically relative, but it is certain that for each scientific ideology (contrary to what occurs, for example, for religious ideology), there is a corresponding objective truth, an absolute nature (Lenin 1979:129).

When we assert that founding the theory of knowledge on relativism means to condemn oneself inevitably not only to subjectivism, skepticism and agnosticism, but also to sophistry, we touch the very core of the problem which concerns us here. Sophistry is the impassable horizon of relativist doctrines, particularly pragmatism and postmodernism. Starting with pure relativism, it is possible to justify all sorts of sophistry, all sorts of cynicism. The cynical views of pragmatism on an essential question like human rights cannot be explained otherwise.

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2

The Alchemist and the Apprentice Myth-Hunter, Comments on Social Engineering in African Social Sciences

Jean-Bernard Ouédraogo & Pierre Bouda

Historians of science often emphasise the social conditions surrounding the appearance and acceptance of a particular reading of the world and the choice of techniques which underlie it. These contextual modalities describe the state of an order of knowledge, of a particular science. While the social dynamic shapes the emergence of an 'objective' approach, as part of a new social need, it always gives a firm steer at the same time to the modes and methods of knowledge towards a specific usage, which is socially legitimate, of an understanding of things which is always a function of the social system.

Although the relations between anthropology and colonialism are still the subject of heated debate, the social sciences, as they are practised both within and concerning contemporary African societies, seem to have escaped this epistemological examination, even though it is essential for them as sciences. It seems that this category carries with it a strong suspicion of inbuilt pedantry. Looking at social demands, as expressed in the predominant historical current of social engineering, this chapter will attempt to determine its influence on both the procedures and the results of social research.

In the field of studies of African societies, this involves a point of departure somewhere between the removal of received ideas (the hunt for myths), which involves the intention to uncover hidden meanings in social practices, and the conceptual and methodological fixing (the alchemy), almost incantatory, of utilitarianism, which a certain standardised usage of 'knowledge' in social matters attempts to impose. Perhaps we will thus be able to expose the issues at stake, as well as the ways they express themselves, as an accompaniment of the social struggle to define the truth on social questions in Africa?

A Second Colonisation of the Facts

The universal celebration of the genetic and spiritual unity of humankind should not allow us to forget the long and often painful way in which man affirmed his difference from nature – hence from the other components of the world of feeling. This has been a complex process of establishing a hierarchy between different forms of humanity. The question of these breaks makes the differentiation on which the essential part of the relationships of men to other natural beings (including man's own stages of evolution) is still based.

The specificity of 'we human beings,' which affirms the uniqueness of mankind, results from a double movement: a distinction of type, by the establishment of an order of classification of all the elements of the world of feeling, and a distinction among the 'self,' by the creation of a cultural gradation, based on an active positivism, responsible for defining the distance from the world of nature. Despite the constant determination of Africans to emphasise the nobility of being black, we must admit that slavery and colonisation have created images that make us reconsider this African humanity, which has been challenged by Europeans, because it has been definitively affected by memories of humiliation, deposited inside every one of us, based on the moral results of a crisis of domination. It can be understood, therefore, that the issue of the relationship of the African to himself first makes it necessary to enquire into the results of the old hierarchisation of humanities on the basis of a reformulated history of the identity of the African of today.

Two aspects of this affirmation, which will soon concern the African continent, should be considered. The first is closely linked to the internal differentiation of men, which establishes a classification of human beings, according to the combined criteria of biological and cultural origin; the second, which is a corollary of the first, taken as a tool of these principles of classification, defines the concrete modes of action to be applied to elements of nature. The Western taxonomic system insists that the European, whose colour is 'normal,' and who enjoys perfection of body and soul, possesses a moral superiority. In 'following all the gradations,' affirms Dr Charles White, Member of the Royal Society, 'we end up with the white European, who, being the furthest removed from brute creation, can from this fact be regarded as the finest member of the human race' (cited in Easlea 1986:297). We should note that at the other extreme of this spectrum is the black man, the African, recognised by everyone as being the still unfinished product of human evolution, as being the primitive or 'natural' person – the alternative name of the African. According to the accepted opinion of scholars at that time, monkeys were at the bottom of the scale, with rudimentary mental powers, followed by the 'orang-utan', the prototype of man and of the negroes of Guinea' (Easlea 1986:298).

In the same way, the process of identifying the second nature of humanity, as established by the philosophers and naturalists of the Enlightenment, leads to the question of the unity of mankind being raised, starting from the diversity and hierarchy among humankind. The definition of the degree of evolution of human society, coming from the 'second birth,' distinguishes between the republic guided by reason

and the state of nature, which in turn is distinguished from the 'savage state,' the realm of wickedness and of the savage, that 'limited and stupid beast.' The criteria which define the advance of reason are in accordance with 'the local economy of nature' (Scott 1990:27). Men, just like plants, according to the scheme of understanding then in vogue, are controlled by an implacable logic that continually creates a range of exotic elements, according to a hierarchy established in the West. So-called natural law, based on physical criteria, legitimised the conquest of non-European peoples, who were classified by the then accepted raciology, according to the degree of resemblance of their cultural and physical aspects to the ideal type, as it was established in Western thinking. Naturally, the shock of the primitive, by raising the question of the level of humanity enjoyed by each new group of people encountered, led to the idea of progress and of how to admit all the members of the human race into the great family of human civilisation. This theological debate became biological and cosmological, and then drifted toward a geographical awareness of the world. The fables of geography, inherited from the Ancient World, were gradually replaced by a concrete geography, fuelled by accounts of travellers. The concept of difference poses a problem for the West, and calls for a solution that could be both practical and symbolic at the same time.

The principle that encouraged and organised the colonisation of Africa was the superiority of whites over blacks. Racial hierarchies went together with colonisation. This sense of superiority expressed itself in the free use of zoological terms in discussing the blacks. The comparison with animals, which this led to, made the natives – 'this savage race' – into absurd and ugly monkeys and gorillas, or into faithful and lovable dogs, or else into swarming insects, just like the huge and incomprehensible world of those colonised. Colonisation, both in its ideology as well as in its practice, was based on this monkey-like conception of the black man, who was firmly relegated to the animal kingdom. With respect to the colonial activity of France, Jean Loup Amselle (Amselle 1996) has clearly demonstrated the role of 'republican raciology' in French colonial expansion in Africa. Colonial ideology devalued the native, and exaggerated natural elements as the source of wealth. The white man who penetrated into Africa kept a great distance, both physically and culturally, from the African, who remained a savage being. Anthropologists who specialised in colonial Africa held and spread a view of a continent that was inhabited by beings prone to spontaneous feelings, the very instinct that kept them permanently relegated to the animal kingdom.

In just this fashion, Griaule (Leprohon 1945:185) wrote about a film made on the Dogons:

The shots were all taken live, just like a newsreel. You can't expect the natives to stage a performance or even a rehearsal. Everything is spontaneous with them, and if you bother them with details, they're lost... The Dogons are wonderful actors. They all have the instinct of public theatre, and every one of them has his own way of reacting and expressing the feelings that move him.

The animal nature of Africans is also referred to in order to explain the consistency of their innate behaviour. In the early days of the colonial conquest of Africa, one of the officers most involved, A. Baratier (Ouédraogo 1991), expressed the motives that lay behind the civilising mission of France in florid language, without mincing words. In the black woman, he saw the 'luscious fruit of Sudan,' while the local people were 'these immobile natives, their height exaggerated by a long *boubou* of blue Guinea material, watching the steam train (progress) go by, wondering perhaps what had brought us to the Sudan, understanding our activity as little as their lack of it.' These natives without needs, desires, or activities were not men. These Blacks were not members of the great human family, and knew nothing about love: 'How could Blacks know anything about love? They don't even know how to express it. They do not have this softest of all words. They do not copulate or only copulate like animals.' So completely absorbed into savage nature, the African is seen as a natural element in the development of the conquered territories.

This animal comparison affected the 'scientific' perception of African social realities. Paradoxically, the confusion of the native world, likened to the animal kingdom without any qualification, but potentially unmanageable, emphasised its dangerous side. Based on this, external domination looked for essential points that would allow for better control of the people it dominated. The themes based on relationships, kingdoms and brotherhoods were responses to this imperial command. The need to control bodies driven by primary 'mentalities' led, on the other hand, to an oversimplification of indigenous activities and to an unjustified reduction in the diversity of local social practices. One can, therefore, imagine the influence of this perception on the 'object' of African social sciences after independence. There was no need for a complicated methodological structure nor an advanced conceptual combination to arrive at a profound conclusion about a reality that was very simple. This habit of looking at things made the native at best an informed witness, an innocent relay for European thought, but never a scholar of his own society, whose nearness to him exacerbated his already highly subjective approach and destroyed any scientific objectivity. Without any possibility of appeal, access to European areas of activity was completely denied him. Any unthinkable autonomy was regarded as the undesirable beginning of some dangerous subversion and to an unjustifiable claim to moral equality with the authorised representatives of the wise old civilisations.

There is no point in revisiting the well-known influence on anthropology of this conception of African societies or about the ways in which the results of 'indigenous' studies were used. We should remember, however, that this period of colonial domination, confident about its old racial classifications, denied Africans any capacity for practising conceptualisation or for formulating any conclusions about nature with the help of reasoned arguments. Based totally on instinct, any reflection from a distance or any scientific objectivity were both foreign to Africans. We now know how far this discriminatory ideology was itself based on a falsehood. The results were disastrous. Left to colonial anthropologists, African societies have only recently become the subject of investigation by a very small minority of local researchers,

who are trying to develop an autonomous academic space. Since the beginning of contacts with the West, any attempts to develop local knowledge have been systematically ridiculed and those who tried to create them denounced as charlatans. Differences noted in the way of implementing colonial domination (assimilation on the French model and 'indirect rule' on the English pattern) change absolutely nothing in the deep denial of local knowledge. Research institutions (universities and research centres), inherited from the colonial period, even though now transferred to local administration, have been decisively influenced by the weight of the past, the damaging consequences of which still compromise the function of science in the development of African societies.

We should recognise that in indigenous society itself, knowledge had to serve the needs of daily life. The circular universe of local philosophy was incapable of creating a distance between a man and those around him, so that an individual who was detached could exploit the world for his own benefit. Knowledge thus appeared in systems of this kind as a guide for daily action, based on experience and on approximations that could still be of use in this framework. There were two reasons for seeking knowledge. The first was the day to day functioning of the social order, which called for well-established prescriptions, of forms of knowledge that wore well and could be repeated, in order to take on the force of law and commonly accepted rules. The second was the need to master the surprising onset of disturbances to the natural order (rain, thunder, death, etc.). To do this, specialists in danger control and maintenance of good relations with the ancestors were established. Even though the technical function of myths (Abelès 1976) has been well analysed, this activity in the technical field is relatively limited and effective. The world of magic does not lend itself to criticism and experiments, which African pharmacopoeia uses with a greater or lesser degree of success. Magic, myths and traditional medicine are certainly directed towards the exploration of the unknown, but they remain enclosed by experimental reflexes, which have little capacity to construct theories and propel social change. This intellectual environment is not a fruitful inheritance, from which a new generation of researchers trained in the Western epistemological world could draw much inspiration. In any case, all the old figures of African intellectuals were rejected by the colonial powers who occupied Africa up to the 1960s. The opposition between the two logical approaches led to an irremediable rupture, and to an unexpected strengthening of an original science that was really African. Even today, a number of researchers on the continent refer mainly to the body of Western concepts, the famous 'colonial library' of Mudimbé, to write about African realities.

The stated intention among the elites, immediately after independence, to defend the history of the logical heritage of Africa, as a reaction to colonial negation, was soon overtaken by a general move towards compromise, thanks to personal ambitions and to a gradual deterioration of scholarly activities. There followed a break up of learned institutions and a redirection of vocations towards a search for political benefits. Alongside these benefits began a ruthless struggle to acquire wealth and

also an increase, more or less virulent, of competition among researchers. The social sciences provided a fertile ground for such rivalries to develop in the search for material resources. Thus on the one hand while at the time of decolonisation teachers and other interpreters lost the social weight they had earlier enjoyed from their position as intermediaries, on the other hand the new emerging elites, who believed for a time that they could reproduce locally the Western academic tradition, soon came to realise that the function of researcher was undergoing an irreversible process of decline in the social scale. During the 1980s, the illusions of an intellectual career evaporated with the deterioration of the material conditions of life for a researcher, stuck between high social capital and seriously diminished economic capital. The institutions with the responsibility within the academic tradition to take on this work of the intellectual and social advancement of researchers either no longer existed or were in a state of advanced breakdown. Other organisations were gradually taking their place and would establish new standards. New forms of collective action, represented by international associations and organisations, still needed to justify themselves intellectually and required means to measure trends in social change, so as to minimise conflicts as they arose. One could well expect that in our market society, every activity had to be based on the principle of financial gain, money, the new mediating factor in social life.

The devaluation of research was a result, in the first place, of a poor level of homogeneity in the local criteria for scientific ability, which could barely establish themselves as a central social value. In the world of 'experts,' individualisation and extraversion were defined from the outside, blocking communication among researchers and maintaining and even strengthening personal faults as well as rivalries over the control of the scarce resource: money. All means were used to keep open and expand the networks for making the researcher more marketable. This resulted in a situation which increased the subordination of local scientific practice to arrangements that had little to do with the needs of learned enquiry. This subordination of science, as a procedure for revealing the facts and means of production of a local identity was, in reality, an insidious colonisation of African facts and problems. It is by setting the questions itself about what is blocking its progress that a society takes charge of its own destiny. Knowledge about the world, which constitutes science, is a powerful means for collective emancipation.

A brief typology enables us to distinguish two main trends among researchers. Most of them take on consultancies and join all the devotees of the 'sociology of development,' which some hostile critics call ironically and with not a little condescending jealousy, the 'sociology of the digestive tract.' This operational sociology, which directs its energy exclusively to 'development,' is hostile to theory, on the grounds that it already possesses *a priori* the great model that explains everything, that is to say 'development.' It advocates a sort of 'social engineering' that is meant to go along with the 'basic development' of the people, who should 'look after themselves.' A minority, it must be said, takes as a starting point a highly academic concept of research and of the legitimate ways of validating it, and holds that

research into the basic mechanisms of society is a priority, which cannot be achieved except by the use of rigorous scientific methods. The researcher in this idealised classic world is in charge of the whole range of his work.

Innovations, Effectiveness and the Control of One's own Time

The aforementioned perspective is contrary to the gospel of proximity with the 'target populations', which explains why a presence on the ground is exalted, even when there is no epistemological justification for it that can accommodate all the controlled variables of systemic 'approaches' of all kinds. Starting from a pronounced holism, which reactivates the tribal vision of African social realities, we rediscover the 'paradigm of actors' (Long 1994) as an epistemological basis for development research. These phantom paradigms, often built on reputations and beliefs, turn round in the development world like profitable prayer-wheels. Novelties appear under such conditions, like so many forms of 'cultural' management of the valuers' market.

African intellectual activity thus appears to be something like a market of second hand or recycled goods. Conceptual objects are seldom new when they are imported and they are hardly ever invented here. To add some credibility to a social activity with staged intentions, this practical system adopts a rigid methodology as a way of fighting against academic theorisation. From the ordinary viewpoint, this asks too many questions without getting many answers. We need answers, even wrong answers – expert investigation will look for the error. But the heuristic power of this domain is extremely limited, in many cases, by the constraint of a methodological monism, which blocks the discovery of anything original and imposes an ideology of local demand that is supposed to be authentic, sacred but still pre-fabricated. In the background, one can detect the implementation of a modernist model, which is based on a bipolar and static perception, expressed in terms of what is 'modern' and what is 'archaic'. The calm application of these approaches leads to a sterilisation of the scientific approach. But for us, the question is still one of defining the way in which knowledge of the social world can contribute to the development of collective well being.

Even if the declared objectives seem similar – the ideology of the human rights will soon be two hundred years old – it cannot be denied that the methods of implementation are often sharply opposed. This contradiction necessarily calls for a definition of the social function of a researcher and of his scientific undertaking. Taking on this classic function limits the researcher's collective usefulness as such, while involving him in a new social life, directed solely by an economic purpose. This accusation of 'notability,' applied to the researcher, who enters the political arena under the cover of his scientific 'competence,' becomes a trader or a 'business patriot' (Maran 1938) leads him on, through one compromise after another, towards a radical opposition between his own personal interests and those of the group to which he belongs. It is worth noting that family pressures, to which many of them quickly give way, do not prevent remorse and other personal concerns connected

with this often painful contradiction. Personal solutions of keeping up research activities are rarely successful in the long term, or at least impose an unbearable social cost.

It is clear that the subordination of scientific work to questions of money involves important epistemological consequences and inevitably compromises the validity of any sociological analysis. In obedience to the commands of development institutions, this so-called 'interventionist' sociology abandons all scientific rigour and uses the most fanciful kinds of methodology, in keeping with the wishes and moods of those who commission these 'studies,' which are in reality simply an expression of 'interventionist policies' translated into pseudo-scientific terms. The field of sociology, which includes a whole area of interactions, is thus transformed into meaning little more than 'social assistance' and/or a scientific excuse for social projects.

This state of affairs is based on a confusion between the social sciences of those trained as engineers and those of 'basic' researchers. The result is that what should be a symbiosis between fundamental research, which leads to fresh awareness, and engineering science, concerned with putting into intellectual practice serious questions about the local value of knowledge, is converted by the sociologists whom one consults and by evaluators of all kinds, who favour a radical empiricism, into an arbitrary separation between complementary practices, and also – paradoxically – a drawing from the 'public domain' of items of sociological knowledge that serve as a learned decoration, which give an illusion of immediate clarity, but can lead only to erroneous and useless applications. The programmed blockage of development policies is also based on this artificial exploitation of 'information'. Yet, to set oneself up against this unduly narrow kind of utilitarianism is something akin to 'madness', so strong is the social power of this 'new science' and so expected is its final triumph.

It seems that this trend, which encourages an instrumentalist reduction of the link between information and action, that is to say one that firmly turns the increase of knowledge about society into a 'technical' treatment of social reality, also has the aim of dispossessing and, therefore, of removing from local society its own means of increasing knowledge about its own development, based on its own ability to achieve this. Jean Copans notes this confusion and deplores its results:

There is straightaway discrimination between knowledge said to be created on the ground and knowledge that really is created in a scientific way. This 'secondarisation' of research work is encouraged by the fact that the 'patrons' allow themselves to be increasingly corrupted by contracts for studies and consultancies, both national and international, and both public and private. Multi-national co-opting is alive and well: there is no time left for basic research or applied research that is overtly public (Copans 1990:319).

This instrumentalisation of scientific practices conceals a form of social domination. The aim of this critique is to complete a perspective of knowledge of the social world, which does not regard action as the carrying out of instrumentalised

knowledge, and which does not consider that the activities that a society undertakes for itself to make its own history can be reduced to some kind of system of technocratic regulation, ordained by outside domination and aimed at imposing as a future system, one that is necessarily based on the unbalanced functioning of the present version. Scientific work, as one understands it, requires an absence of time constraints. But the person who commissions research is in a hurry to use his time to make money. This means an emergency use of scientific practices is so necessary which makes nonsense of the cautiousness that all experience of the past indicates.

Plato, for example, makes a point of insisting that 'a free man always has time at his disposal', to examine whatever questions it occurs to him to consider. At the other end of the scale from Plato, Feyerabend commented ironically that both the slave and the expert have to follow the timetable of some superior, who sets out the list of questions they have to consider and points out the time still left to them by the inexorable hourglass (Feyerabend 1979:63). Here is a problem that seems at first to be of no importance. One always has the impression of being able to arrange one's own time and also of time being, in any case, a necessary constraint, the disagreeable aspects of which one can never avoid. Still, Plato (and Feyerabend as well) implicitly make one think that an expert or a slave, from the moment that he is no longer in control of his own time, can probably no longer avoid making statements that are unduly hasty and erroneous. This may seem to be a peculiar statement, but it nonetheless concerns an essential dimension of conditions for undertaking research. Haste is a more significant handicap than is generally believed. One should not be in too much of a hurry, not harried by external time pressures, when one is involved in research. As Whewell frequently insisted, there has to be a 'eureka' in any discovery, that is to say a moment of fortuitous intuition. Until this wonderful moment, time is needed for questions to ripen, for 'fortuitous connections' to be made, and for helpful analogies to be discovered. In the studies which we are discussing, constraints linked to the pressure of time limits, imposed by the need to act, operate in the direction of making it impossible for the researcher to reach any conclusion that is consistent with the caution required by the scientific method. Faced with the need to produce results within a certain time limit, the researcher gives up the slow pace of a rigorous testing of hypotheses. This encourages him to take positions that are 'reasonable,' which means to take commonsense positions, and present them as the result of methodical research. There is a twofold interest in making commonsense statements: first, they are immediately available, because they are given statements and do not have to be constructed; secondly, and above all, they at once set up the idea of 'commonsense,' something that can immediately be agreed on. This spares the researcher from having to make a laborious effort to justify something in order to make it appear to be true. For those who want 'to get on with things,' it is sufficient to throw together a 'scientific presentation' – or in other words, to set out conclusions in the formal framework of ritualised rhetoric.

One can see how great the damage done by this is:

1. Science no longer has the aim of discovering the truth. Research lays down other aims or perhaps it pursues no particular aim at all; and
2. Science has to follow the rules of common sense, and so the critical spirit, which should in principle control all scientific research, is regarded as a source of endless anxieties, and is thus rejected as vain and useless intellectual folly. It is hardly surprising, therefore, to see methodological protocols, designed to contain a break with spontaneous presentations, put to the service of a business whose aim is to endorse prejudices by promoting them into authentic scientific information.

Wittgenstein wrote that in grammar, there are no minor differences. Small variations in grammatical rules have the result of producing substantially different languages. In the same way and *mutatis mutandis*, one can say that in scientific activity, nothing can be left out without consequences. In particular, one has to take the time necessary to apply the scientific method in all its rigour. To the tyrant, who asked him to produce a rapid way, easy but sure, to learn the rules of geometry, Archimedes replied, 'Sire, there is no royal road to geometry.' There is an urgent need to believe that in the social sciences too, there is no 'royal road' to reach anywhere that is worth reaching. If scientific research is ruled by practical requirements that militate against scientific principles, it will no longer be able to convince the community of the validity of the results it has reached. In particular, the need to observe time limits that cripple the rigour of the research makes science run the risk of seeing the principle of moving away from any preconceptions collapse in the face of the practice of sticking to spontaneous presentations.

In the face of the failure of academic institutions and of the growth of the social crisis, the patient deciphering of social realities is giving way to another way of carrying out research, which will be turned into widespread research for easy gain. Good intellectual intentions will soon give way to commercial pressures.

From External Proof to Blind Actions

Epistemological prejudices affect both methodology and areas of investigation. The wise observer of the African research world soon learns an almost systematic and one-sided definition of research themes that are 'interesting' for wealthy sponsors, for 'experts' and for international organisations. These organisations and 'partners' constantly interfere to define a hierarchisation of problematics, which often has nothing to do with the concerns of researchers themselves and even less to do with any local perspective of building up and using information. In this situation, the African researcher is reduced to being nothing more than a collector of 'facts' on the ground, for the 'partner from the north' to analyse and to write in a language that is suitable for such raw material. This skill moves further and further away from the spirit of discovery, and its arrangements are made in accordance with market forces. One cannot say often enough that the symbolic benefit of academic recognition is devalued in favour of the doubtful advantages of acquiring material possessions.

What legitimises this ‘mercenarising’ of scientific representation is, according to Etienne Leroy,² ‘a mixture of scientific knowledge, bureaucratic skills, political blindness and ethical irresponsibility...’ The author asks himself the question: ‘Is this the sign of maintenance or rebirth of a connection with colonial domination?’ Every ‘African’ science runs up against strategies for neutralising its efforts to set out its own theoretical and methodological requirements, in accordance with its role of revealing the hidden meaning of the social future, the leading instrument for progress. ‘Freedom is to know what is needed,’ Engels said somewhere. It is clear that we should not claim a special scientific status for African societies, but rather state the need for a local and ‘objective’ relational configuration, which is necessary for all sociological distancing. This proposition is essential for any untested sociology, imbued with a relationship of ‘equivalence’ for untested methodology, of some happy partnership with falsely fraternal aspects. Scientific domination imposes its own laws. ‘The categories that limit discourse created particularly by social relationships. These impose their own space by declaring it to be rational and neutral. They put forward the idea of reciprocity, where they alone hold any power, and they say to those who challenge this reciprocity that they lack objectivity’ (D’unrug, Moreau de Bellaing 1982:133). We shall try to show below how and to what extent the triumph of this kind of logic can be overthrown. It soon appeared to us that the career of sociologist cannot sustain this monopoly and this social neutralisation of the social science researcher’s ability to shed light on things. An enlightened scientific practice involves also a complete refusal to listen to these monologues on African realities.

Just as the work of the researcher can be a source of power, so too must he bear the heavy burden of defining the interests he serves. Whatever choice he makes, one thing is certain, his choice will not be without consequences. It will make its contribution to the construction of the city. One must still recall that it would be an illusion to believe that scientific practices are immune from all debates, or from subjective views about social conflicts in which the researcher is involved. Judith Schlanger interprets this necessary engagement when she writes:

As far as the desire for knowledge is concerned, it is involved in the clash of interests, and it is mixed up in all kinds of interest. Indeed these interests make use of it, but it also derives sustenance from them. They distract it, but it distracts them also. Just as it is their mask, it is also their parasite (Shlanger 1979:9).

And in the midst of its solitude, this over-determination concerns what she describes as the ‘deep thinker.’ At any rate – and this leads us back to the thinking of Elias – the influence of the milieu or of the collectivity in formulating scientific dialogue carries with it a dialectical argument: the ‘externalist’ dimension simultaneously takes on its opposite, as an inevitable reflex. In the scientific discussion, Judith Schlanger goes on to emphasise: ‘As well as understanding what is immediately under consideration, it is also necessary to state one’s own position and point of view. A problematic has its own problematic, in its cultural dimension’ (Shlanger 1979:14). The social analysis that enables a researcher to ‘see himself as he looks at himself’

settles the problem of his ambivalent social position as an engaged observer. Aware of this situation, the researcher can adjust his aim, so that with this correction, he no longer fires blank ammunition. This reflex is the one condition of any effective reasoned and enlightened action.

Technicist Illusion, Ends and Responsibilities

The basic question seems to be the following: what is the purpose of knowledge? If it is simply a question of given facts, available for immediate exploitation, then knowledge crushes and concretises and is just a matter of 'preconceptions.' On the other hand, reasoned and problematic judgment is helpful when it contributes to the creation of conditions for social well-being and where, to do this, it seeks continuously to define the terms of its own existence, as a collective subject of social change (Cornu 1997).

The consequences of this engagement are in large part dependent on the intellectual evolution of the society in question. Elias indeed explains that social science becomes a possibility only at the state of development where there is a clearly stated transition from self-constraint to self-control. One is aware that Elias's theory states that any scientific activity worthy of the name depends on distancing as a factor of 'emotional disenchantment,' which makes autonomous both the object and the hidden social relations behind it, which give it form. The privileged position of the researcher, if it does not lead to his transformation into a sterile intellectual elitism, can only be understood by setting it into the evolution of its social context. The social framework for research in Africa, whose sociology has still to be carried out, has not yet set out a formulation of the need for a scientific control of the human and natural environment. Uniformist categories seem to dominate these research perspectives. The consequences of an attitude of this kind do not yet seem to present a visible threat for individual and collective existence. The extraversion of ways of life and of social control by dominant groups leads us to neglect, or rather not to see, the complexity of local realities. These can appear, when observed from the point of view of action, to be without contradictions, especially when seen through the over-simplified classifications provided by colonial ethnology and the most radical theories of development. It is thus that

the way in which the individual members of a group react to everything that affects their senses, and the significance they attach to their perceptions, depend on their depth of knowledge and, therefore, on the degree of conceptualisation that, case by case, their society has reached in the course of its development (Elias 1993:12).

The power of the process of social disqualification that transforms traditional native knowledge into a matter of folklore does not leave unaffected current efforts to construct references that compete with those that have been imposed. Historians of science often repeat that a social 'ambience' propitious for scientific progress is a necessity. The 'pre-scientific' context of the current situation shows that the alchemist

is not just a creature of metaphor. As A. Koyré shows, the alchemist is the central character of the 'next world.'

It is not the material impossibility of carrying out his tasks that halts the alchemist. He does not make use of them, even when he has them all to hand. It isn't the thermometer that is missing. It's the notion of heat as something that can be precisely measured. So he is content with terms taken from common experience: a fierce fire, a slow fire, etc., and he makes no use or hardly of a -scale. (...) That is precisely the reason why the alchemist does not use one. If he did, he would be a chemist. And even to have the idea of using one, would make him a chemist already (Koyré 1971:350).

And so one can see why he limits himself to speaking in vague terms. For him, the meaning of science is not the search for the unknown. It is the 'technique' which he hesitates over, while the object of the exercise to which it would be devoted, is already known to him. The experiment is sovereign. Koyré emphasises later on that modern applied science is characterised precisely by the control of the practical by the theoretical. Accepting a certain model of society, this positivism exalts the carrying out of transformations by the use of 'applied tools', which then become ends in themselves. Y. Schwartz thus rightly observes that 'Rationalisation does not become degraded into a technicist illusion, except to the extent that it starts to neutralise or to consider as predetermined the questions of the ends for which it provides its faculties for arranging things' (Schwartz 1995:113). The least of heuristic preconceptions is to refuse any end that is not hypothetical.

The African researcher, either as a happy clown or as a sad and obstinate Prometheus, painfully tries to master the dilemma that overwhelms him and to refuse to retire into internal exile or to deny his own identity. Forbidden any access to realities, the apprentice alchemist has only one eye fixed on the almost mythical search for gold. 'Gold alone is the object of his desires. Gold is his legitimate son, because only gold can be a legitimate object to produce' (Eliade 1990:19). From the epistemological point of view, therefore, we find ourselves in an exclusive and enchanted relationship, that of the 'Golden Mirror gazing at the Golden Mirror' (Eliade 1990:19). At a basic level, the history of the alchemist who sells the 'common soul' defines in miniature another scale of virtues, which claims to proclaim a counter-discourse, this time a political one, and to define a new episteme. 'Scientific production' has something in common with social life, and the contradictions contained in the latter are reflected in scientific activities. We have reached the moment of decision. The difficulties in the way of developing scientific activities that are directed at local problems come from an increasingly widespread socialisation of knowledge and of the stakes involved. 'The socialisation of science,' writes J. Bonitsier 'complicates its ethics. The proper ethic of science, that of wise scientific activity, is increasingly mixed up with the ethic of the man of science's responsibility towards society considered as a whole' (Bonitsier 1997:180). It is obvious that 'society' is divided by contradictions and that these divisions affect singular identities. Debates are conducted

and scientific activities defined around the philosophical definition of social perspectives. All philosophies can claim to be legitimate at a strictly formal level. But it seems to me that the line of ethical division includes the researcher's responsibility and also his methodology and therefore an historical appreciation of science. The alchemist is set on his need to understand final ends – the 'eternal truths' – and tries to impose a dogmatic view of the social space, reinforced by a certain moral perspective that also sets out a vocation of scientific practice. When faced by this concept of scientific practice, we can soon see how monolithic it is. Refusing to examine any social presuppositions, hastily described as 'sociological,' some developmentalist approaches do not acquire the means of affirming a methodological autonomy that is essential for any investigation of the social order. They conceal more than they reveal. But the principal vocation of the sociologist, according to Elias, is to hunt down myths. He writes, 'This hunt for myths, the denunciation as worthless of the contents of myths which underlie presentations – that is the task of science...' (Elias 1981:58). This function of science is not compatible with its diversion into channels of gain, which creates an illusion of intelligibility and of practical effectiveness as a coherent system for immobilising the social dynamic.

Universalism, Objectivity and Disinterestedness

There is a need to formalise and to theorise, while always maintaining a constant relationship between abstractions and facts. As M. Callon notes, 'The scientist never works directly with nature itself, but only with representatives of nature that are more or less faithful, and more or less numerous and remote' (Callon). It is nonetheless true that too great a tendency towards speculation risks confining the sociologist to unduly narrow limits, to a kind of glossy but fruitless narcissism. Do we have to emphasise that the distinction between *learned sense* and *common sense* is what forms the social sciences? It should be underlined that the scientific practice of sociology necessarily links together demonstrative and conceptual logic with empirical facts. Elias asserts that 'Sociological theories that are not verified by the work of empirical sociology are worthless. They do not even merit the status of theories.' To sustain this perspective is an invitation to modesty in the face of the complexity of the social world, which never immediately reveals the logic of its working. The concrete is also a complexity that some 'Africanists' obstinately refuse to apply to 'African matters' – even such a simple human relationship as that of the famous local 'networks' would clear up this point.

One may have the impression that some of the 'commissioned studies' in social sciences reintroduce into a study of matters human the classic distinction in natural sciences between basic and applied research. Alongside research that is concerned solely with discovering the truth, whose only aim is knowledge for knowledge's sake, there are studies that are carried out by teams and in laboratories financed by business. We know that such research has rendered important services to science, and also how much the great adventure of computer science owes to research carried out in the United States for military purposes. It is undeniable that today research

into the science of matter cannot easily do without private financing. And since human sciences receive less attention from governments than hard science, we can look favourably on the search for help from development institutions for studies which they finance about problems which they have defined. Is this not the way for the social science researcher to have the certainty of getting a grasp of the good and the ill of mankind? Is this not the only way that the science of man can prove, according to a famous formula of Marx that its theories are of this world? One also recalls the words of Auguste Comte: 'to know is to foresee; to foresee is to act'; and one can argue that studies commissioned by development institutions are an effective way of fulfilling the destiny that was appropriately mapped out by a founding father.

There are a few considerations that can help to moderate any such optimism. In the first place, it is clear that we are not dealing here with structured teams, working for the long term. In the natural sciences, there are properly integrated teams, enjoying a certain autonomy, and also enjoying adequate means and time. Competition can certainly sometimes impose time limits, which may, to a greater or lesser degree, be incompatible with the methodological precautions that would guarantee scientific objectivity; but without some reasonable time limits, one would hardly be able to reach worthwhile conclusions in any complicated research. In the research which concerns us, however, we are often confronted with what are rightly termed 'ad hoc teams,' assembled to deal with specific programmes, in such a way that their research experience cannot be built on by the wider group. Against this background, any new research often has to begin again from scratch. This kind of research seems, above all, generally to be carried out in a context that vitiates scientific norms as defined by Merton (Merton 1957): universalism, communalism, disinterestedness, organised scepticism and autonomy. These norms, which form what Merton calls the ethos of science, were subsequently submitted to more or less severe criticism by authors who fostered the relativist current in the sociology of science (Barnes, Dolby 1979:3-35). In a nutshell, they blamed Merton's point of view for idealising science and not treating it as a human activity like any other. For such critics, science was culturally determined both by its institutions and by its content. As far as we are concerned, we have to consider science as a specific activity, which needs to be examined through the characteristics that form its identity. And it seems that the norms defined by Merton touch on something that is fundamental for scientific enterprise.

According to Merton, the principles by which scientists control their actions form what he calls their 'scientific conscience.' Linked to the principle of autonomy, this means that the man of science has to reply to his conscience alone for having done work that is rigorously scientific. It seems, however, that the area of 'commissioned studies,' with all its networks, its time limits, its epistemological compromises, etc., imposes other requirements on research than purely scientific ones.

Merton defines universalism by the idea that scientific statements must be subject to previously established and impersonal criteria, and should conform to already existing observations and knowledge. This way of looking at science has prompted

the idea that it is deeply theoretical. The idea of criteria and of pre-established knowledge goes in the direction of a specific culture. Scientific research should be conducted in a particular cultural environment, which confers on it the cachet that is its due. This is not a question of jargon, but of an approach and of a group of concepts that contribute to the definition of a community, and form the bases for the possibility of an accumulation of progress. One of the requirements of a scientific culture is indeed that a corpus of scientific knowledge can be built up. Without any such dynamic for recording knowledge as it is acquired, we are destined to stumble uselessly around. In the social sciences, circulating concepts seem to be of fundamental interest because concepts can pass from one researcher to another and acquire their own autonomy and a validity independent of the context in which they were originally formulated. In addition, it is only through such a process that they can be made more precise, more refined and, in the end, better controlled. It is particularly interesting to see concepts used in the context of one study being employed in an illuminating way in a completely different area. Since in the social sciences, a rigorous replication can hardly be envisaged, this process can be regarded as a test, which can provide an acceptable substitute for an experiment. By demonstrating the proof of its heuristic validity, a concept puts itself forward as an illuminating tool for investigating a particular field of social reality.

One can thus see that scientific culture is an area in which the researcher is talking to the scientific community. It is not simply a methodological ritual that places the work of a researcher in a given scientific space. A knowledge of and the use of certain concepts are also highly necessary. In the type of research we are talking of, the 'practical' spirit can often become allergic to any effort to theorise. Men of action who are in charge of development institutions are mistrustful, sometimes almost pathologically so, of anything that they profess to regard as useless and empty discussion. This is why the researchers who work with them soon learn to be 'practical,' and turn away from any subtleties which might complicate matters, when what is needed is something simple that makes action easily understood and effective. One can thus often see the methods of social scientists reduced to a collection of recipes to 'find' what the men on the ground need to conduct their work. One can understand the remarkable phenomenon that the development studies we are talking about use, without any critical hesitation, the concepts worked out by the technocrats of development institutions, which they then impose on others as so much popular jargon. In this way, such concepts as sustainable development, sustainable human development and good governance, etc., are used by researchers, who never query their relevance. It is seriously stylish to use them again and again, often with a complete lack of clarity. If the Mertonian norm of communalism emphasises the idea that the results of research should be a common good, it implies equally the need for an appropriate use of concepts by the scientific community. One could almost say that scientific concepts only wear out when they remain unused. But as one can see, development research leads to a culture that does not encourage the creation of a useful heritage in the field of social science.

Accepting uncritically concepts put into circulation by the development institutions, for practical rather than theoretical reasons, runs against the norms of disinterestedness and organised scepticism. It is indeed clear that important financial considerations explain the collusion between social science experts and development technocrats. The principle of disinterestedness should obviously be at the heart of the objectivity that characterises science. Disinterestedness is the fact that the researcher, in his research work, should have no reward in view, whether it be material or moral. This is the guarantee of emotional neutrality and, therefore, objectivity. The 'operational' social sciences, which are only a form of ideological social immobilism concealed by claims to practical effectiveness, on the lines of 'social engineering,' are in fact the expression of a negative shift in the capacity of African societies to take charge of their own history.

In a work devoted to inequality in the face of science throughout the world, Charles Morazé emphasises that wherever science has been able to develop freely, it always operates in favour of equality. He sees equality demonstrating itself in two ways in this context. First, it brings together scientists from every geographical and racial area and from every level of wealth in the 'universalism of one and the same logic.' And subsequently, it contributes to the improvement of living conditions for all people. This point, indeed, corresponds more with results from the so-called exact and experimental sciences, but it also fits with the social sciences. He then makes a highly penetrating remark:

The consequences of this are particularly serious for developing countries. The less they are present at the cutting edge of future scientific development, the lesser their chance of making their point of view prevail in the scientific community. The more they are dependent on decisions taken by external authorities, situated at the poles of discovery, the more they will suffer from the deficiencies of being at the periphery. Science is always a herald of the future, but it builds this future on what has been acquired in the past and on decisions which, in the last resort, are also based on ambitions and interest inherited from the past. Developing countries have so much the less chance of using science for their own benefit, because they have themselves been the victims of a history that has particularised the progress of knowledge (Morazé 1979:5).

This recalls the principles that direct scientific activity helps to underline its importance for society, but more especially to show that its performance depends largely on the conditions in which man has placed it. There is nothing fatalistic about anything that has been said here. In the face of the deteriorating conditions for scientific endeavour, individuals create new strategies, consisting mainly of giving up its practice and in turning to other, more lucrative, activities, political ones in particular. These strategies are also to be seen in the movement of people to brighter horizons. The brain drain allows individual scientific projects to be carried out after they have been thwarted by deplorable national conditions. Getting around such blockages might

happen in a utopia which minimised all constraints and exalted abnegation and tenacity. Do conditions exist in Africa on which to base such a perspective? Perhaps they do. But in either case, is it still possible for Africans to do otherwise? Basing themselves on their pride on what they have acquired in logical and practical fields and by remaining sufficiently modest about undergoing fresh apprenticeships, African societies may perhaps reach the point of marking out new ways for a prosperous future.

Notes

1. A French technical assistant, who announced himself to be an 'expert in social sciences,' insisted, without any reservations, that African students should justify their theses by experimental work on the ground. His idea was to strike down African pretensions to the plane of abstraction. One original way of realising 'The Great Divide', that has been so well analysed by Bruno Latour. See in particular his article 'Comment redistribuer le grand partage', *Revue de synthèse*, n° 110, April-June, 1993.
2. 'L'expertise internationale en Afrique: le cas de l'expertise juridique sur les questions foncières', *Bulletin de l'APAD*, n°2, December 1991, p.16. The new wave of development anthropology, a new subjects for experts, concerned with the material rejected from 'populisms', maliciously raised the question of the third kind of researcher: 'to be relevant and to take account of the fact that they were supposed to involve themselves in specific activities, did not those who embodied it have to play the role of negotiator or mediator between the developers and the developed, and thus press on the needs of their activities to the very end even if they were not completely on the side of those who were carrying it out?' J.P. Dozon, 'Le dilemme connaissance-action: le développement comme champ politique', *Bulletin de l'APAD*, n°1, p. 15.

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3

An Introduction to the Epistemology of the Social Sciences

Sémou Pathé Guèye

Since their appearance in the 19th century, the social sciences have made undeniable progress, both from the point of view of knowledge that they provide us on social reality and from their practical use in the daily management of society. This is also true in Africa.

We should, however, recognize, at least as far as our continent goes, that such progress runs the risk of being slowed, if not compromised, by what we consider the dominant paradigm in these sciences. This paradigm is empiricism, which we will return to later, and which is all the more problematic because we live in a time of social changes of all sorts which make the subject of these sciences more and more complex, and which requires them at the very least to revisit their tools and approaches.

Reflecting upon science, its procedures, the value and the limits of knowledge which it provides us on the world is, by definition, the vocation of epistemology. But it is also to accept that science can make itself the subject of questioning, still all the more necessary because it happens in the middle of a “crisis,” that the paradigms, theories, methods and concepts that it had successfully used up to now are faced with new problems or “polemic facts” (Bachelard), which crop up in their fields of application, and which impose the need and sometimes the urgency of modifications, of revisiting, even reconsideration, without which acquired knowledge becomes obsolete and new scientific progress impossible. This is, after all, valid for all the sciences, whether they be natural, human or social.

But for the human and social sciences in particular, in addition to the recognized need of epistemology for all the others, we can add other specific considerations that increase this need. These considerations are linked to the nature of their subject as well as the relationship that the researcher himself, who is an integrative part of it, maintains with his subject, which will necessarily have consequences

both on the methodological level and that of the value of knowledge produced. Those are aspects which, as we will see, have implications in the debate on the question of the epistemological status of these sciences, and on the legitimacy of their claim to be sciences in their own right, a legitimacy still contested by specialists of so-called “hard” or “exact” sciences in the name of a conception of scientism based on the model of natural sciences at a particular stage in their historical development.

These are a number of questions that we will be sure to address in this text which is, however, mainly concerned with reflecting on the nature and necessity of epistemology, but also with shedding some light on the concept of science as it refers to a knowledge of a particular type, with the various issues which its definition and practice include.

Epistemology in General

In accordance with its Greek etymology (*episteme*, knowledge, *science* and *logos*, study), epistemology can be defined as the study of science in general. Its objective is to reflect on science, and what distinguishes it from other types of knowledge, on its criteria of definition, on the methods and procedures of the production and validation of its results.¹

Such as we have just defined it, and except in overly extending understanding, epistemology does not exhaust all of the dimensions of scientific knowledge. There are other possible approaches to science, as for example the sociology of science, which understands it as social activity, the anthropology of science, which understands it in its cultural dimension, the psychoanalysis of science, the economics of science, the politics of science, the history of science, etc. Each of these approaches, in its own way, allows us to shed light on science.

There is also the question, too complex to be addressed here, of the relationship between epistemology and the history of the sciences. Even if we do not see how rigorous and concrete thought on science could save the cost of information as precise as possible on the process of the formation of concepts, theories and approaches at one moment or another in the development of scientific activity, there are nevertheless differences in approach between the two.

But epistemology is not only a reflection on science in general. There is also an epistemological practice which, instead of dealing with general problems of science, concentrates on a particular scientific discipline of which it tries to understand the specific problems. It is in this way that we sometimes speak of “particular”, “local”, “regional” or “special” epistemology.

However, whether it be general or special, epistemological reflection is subject to another distinction. We speak of external epistemology when, as is often the case, epistemological reflection is conducted outside of science, i.e. by a non-scientist by training. For example, in the organization of teaching, epistemology is an integrative part of the philosophy course and its practice is reserved in general

for philosophers who are not necessarily trained in the concrete disciplines concerned. This gives rise to a sort of contradiction which leads specialists of particular disciplines to feel – and sometimes rightly so – with respect to this epistemology, which comes to them from the outside and which sometimes gives the impression, not always without justification, of attempting to regulate them – the same distrust as artistic creators with respect to art critics. Such an impression has often pushed some scholars to engage themselves in an epistemological reflection on their own activity, but not always, moreover, with all the success that direct knowledge of their subject could hope to do. We then refer to *internal epistemology*, to be understood in the sense of autonomous reflection by specialists of a given scientific field on their discipline. That can seem, *a priori*, to offer a greater guarantee of relevance to epistemological reflection. Yet, this is the case only if the person in question has been trained to go beyond the simple use of knowledge and instruments of his own discipline, to develop a lucid and demanding critical reflection on them. Unfortunately, this is not always the case: the training of scientists in our universities thinks nothing of such reflection on science which is rejected as related to “philosophy” or even “metaphysics.”

The concept of science in epistemology

In a famous formula, Aristotle stated, “At the beginning was surprise.” Man’s nature is not to content himself with living in the world, passively watching phenomena take place and asking no questions about them. On the contrary, from the beginning, the world “surprises” Man. It presents itself to him as an enigma, a set of mysteries to decipher, fuelling all manner of questions to which he should find appropriate responses, not only for simple reasons of intellectual curiosity but also to be able to survive in a sometimes hostile environment. Where does the world come from and where do the beings that inhabit it come from, and the realities which compose it? Why is there something rather than nothing? How does it come about that things happen in a certain way and not another? Are the appearance, succession and disappearance of events and phenomena related to pure chance or instead causes? If need be, are they understandable by Man? These are the fundamental questions that Man asks himself.

We believe that it is from this point of view that we should explain the existence in all human society of myths, legends, tales, artistic practices, religions, with the objective (though not exclusive) of conferring an intelligibility on the world and the phenomena which take place there, i.e. of providing men with a satisfying explanation of what is happening there, to be able to control it, both by thought and by action. Otherwise, the human species could not survive in a nature that it, of all the species, is the least well prepared to dominate. Despite their differences, all these approaches have a common denominator: recourse to the principle of causality. Indeed, they all start from the idea that everything obeys a cause, whatever its nature may be – mysterious or accessible to human reason.

However, the forms of intelligibility that myths, stories, legends, religions or art propose, give rise to a causality in the sequence of phenomena and events which always remains shrouded in mystery, insofar as it refers to realities which transcend human reason. It is thus through faith, simple belief or collective traditions that it makes its presence felt. After all, this is what explains the unfortunate tendency to reduce these explanations to the simple domain of the irrational. It is justified if, by “irrational,” we mean what does not proceed from reason in the logical sense of the term. But this way of systematically denying – and without other forms of trial – the intellectual approach at work in myths, stories and legends is fallacious if we understand by “irrational” something totally contrary to reason and totally foreign to it.

Indeed, if we extricate ourselves from a strictly rationalist and ethnocentric conception of rationality, these approaches can be considered “rational” in several ways. They are first rational to the extent – as we have previously stated – that they presuppose that everything has its reason, that they obey an open or hidden reason, accessible or not to human understanding. They are also rational insofar as, even if the intelligibility that they ensure is not produced in accordance with rules, criteria and procedures of reasoning judged to be valid according to principles of Aristotelian logic, they do not clash so much with reason. Thus, there is nothing rationally shocking in the idea that instead of being the simple result of the evolution of matter, man was created by God. Moreover, there is not necessarily more logical consistency in the structure of Darwin’s explanation, than in a Dogon or Bantou myth of the origin of man; if the rules of construction of the consistency are not necessarily identical, it is only the narrowness of a view of a certain rationalism which smacks of positivism that sees a “childhood of the mind” (Hegel) in mythic or religious thought.

These considerations would seem to be all the more necessary in that they include implications in the debate on the epistemological status of “endogenous knowledge,” disqualified in the name of the same narrowly ethnocentric conception of rationality and knowledge. They should not, however, mask the fact that “science,” in the precise meaning when it concerns epistemology, remains, despite everything, a qualitatively new knowledge, obeying its own criteria of definition,² having its own procedures of production and validation of knowledge, supposing a certain number of conditions, and evolving according to particular modalities.

More precisely, in the sense where it interests epistemology, the concept of science refers to a system of rational, objective and universal knowledge related to a given domain of the real, which allows us to discern, between phenomena and events which are produced within it, necessary and constant relationships which we call laws, which we can go on to explain what is happening in this field or predict what can happen there and thus be able to act effectively on it. Let us clarify this definition.

Systematic Nature

A set of disparate, fragmented bits of knowledge without any link between them, even if true, does not yet constitute *ipso facto* a science. In order to speak of a science, in a domain of the real, this knowledge must not only be true but ordered and structures in a coherent whole, on the base of principles and of a rigorously developed and scrupulously applied a approach.

Rationality

We have noted above that rationality, except if we understand it in too narrow a sense, cannot be considered an exclusive attribute of science. But the criterion of rationality, as we apply it here to science (itself understood, not in the sense of knowledge in general but of a particular knowledge, of a qualitatively new type), means two things. On the one hand, that science explains phenomena and events which are produced in the world by resorting to human reason as instrument of discovery and analysis, and not to any elusive cause. On the other hand, that the discourse and approach chosen to do this are totally respectful of the principles and formal procedure of questioning such as they are established by logical science.

Objectivity

Scientific knowledge aims to render an account of reality such as it is, which implies that it is the most faithful possible to the nature of its subject and correlatively, that it is the least possibly marked by “subjectivity” in all its forms (feelings, desires, philosophical, political, moral, religious, etc. convictions). We have deliberately chosen the word “aims” because in reality, objectivity, in the sense of total and perfect appropriateness of the knowledge and the reality on which it bears, is an ideal towards which science can aspire rather than an objective which it attains.

Universality

This criterion can be considered as the result of the two preceding ones. Indeed, if knowledge is conceived of in such a way that it is accessible and acceptable by reason, which Descartes termed the best shared thing in the world, and if it is developed in a way be to able, by its objectivity, to realize the “agreement of minds,” it should be recognized as valid and acceptable by anyone of good faith and fairly competent in the matter, indiscriminately of social origin, or personal convictions and preferences. But on this point as well, it is also important to stress that the universality of scientific knowledge cannot be understood in an absolute way anymore than absolute objectivity is possible.

Laws

In the sense that we understand it in epistemology, different in certain relationships from their legal sense, laws are not pure inventions of the human mind even if the mind discovers them, develops them and formulates them. They are inherent to the nature of phenomena and events to which they apply. They are the expression of general, constant and necessary relationships. What do these three adjectives correspond to? First, we cannot speak of a law when we are dealing with a phenomenon or a particular event, or a particular aspect of a phenomenon or an event. In other words, we cannot make a particular case a law or a general process. Nor can we speak of law when we are dealing with a phenomenon or an event which is not able to be repeated identically in identical conditions. We cannot speak of a law when we are dealing with a phenomenon or an event which only happens exceptionally or which, depending on the case, can appear in different forms. Therein lies the consistency of scientific law. Finally, the law always expresses what happens and what cannot happen, what always happens necessarily when we match up phenomena or events in identical conditions.

Monitoring

Even though it is only implicitly included in the definition of science such as we have previously formulated it, this is an extremely important criterion. Science does not accept any idea, any explanation which is not monitorable, i.e. of which we cannot establish the truth or falseness, either by questioning, or by recourse to experience and sometimes by the combination of the two. In other words, there is no scientific knowledge without proof, the development of such knowledge implying the definition of conditions, procedures and theoretical and/or methodological means of establishment of this proof. The importance, even the supremacy accorded to experimental monitoring, i.e. the verification by facts, as we see to a large extent today in scientific practice, including in the social sciences, can be considered a natural consequence of the inductivist approach established by some as the scientific approach par excellence.

Birth and Progress of Scientific Knowledge

How does science, thus defined, form? This question includes two aspects: on the one hand, that of the birth of science, i.e. of the passage from prescientific to scientific and, on the other, that of the progress of scientific knowledge itself. We have chosen to address the first aspect in light of the epistemology of Gaston Bachelard and the second through the debate which divided the two other great figures of contemporary epistemology, Karl Popper and Thomas Kuhn.

But first we need to say several words on the question which has dominated all of the history of knowledge, that of the origin of our knowledge, in particular our authentic knowledge, a question which, as we know, has divided the two major currents of the theory of knowledge, rationalism and empiricism.

The first current cited, which we can trace back to Plato, considers that knowledge results from a free activity of the mind which should be capable of freeing itself from the knowledge that it draws directly from the senses, from rumor, current opinion or tradition which can only mislead it by making it mistake simple appearances for reality. It is the entire “idealist” tradition, represented in various ways in the history of philosophy, with some slight differences which we will not dwell on, by later thinkers like Descartes, Leibniz, Kant, Hegel, to only cite those few. Beyond differences in their respective systems, on the question which concerns us here they share the idea that true knowledge is not given but constructed, acquired at the price of a demanding, methodical and persevering effort of reason and a particular vigilance in order not to confuse essence and appearance, illusion and reality, truth and error.

A second tradition contrasts with this, that of “empiricism,” associated with the names of philosophers like Hume and Locke, for whom all ideas conceived by the human mind are derived from perceptible experience which is thus raised not only to the status of source but also as foundation and guarantor of all authentic knowledge. Outside of perceptible knowledge and “facts” such as it gives us, the rest is only pure imagination. The meticulous recording and the most faithful possible information provided by the senses becomes, for this second tradition, the approach par excellence of science, whereas suspicion of them and their critique was the necessary starting point of all enterprise of knowledge claiming the status of science for the previously mentioned current.

Here, the question is not to know which of these two gnoseological approaches is the most relevant. Behind their seemingly irreconcilable opposition, each of them translates an indisputable aspect of the scientific approach.

If the respect for “facts” such as stressed by empiricism, is the best way to avoid erring in gratuitous ratiocinations, it is also a simple fact of “experience,” that the scientific mind cannot, at the risk of being mistaken, settle for taking the data that our senses provide on the world at face value, or information that we can draw from public opinion, or even tradition. For example, we “see” every day that the sun “rises” in the morning and “sets” at night and this perceptible data establishes itself with such force of truth for all ordinary men that it seems unthinkable to him to doubt this for a single moment. Yet, thanks to science, we know that it is something totally different that really happens. In fact, beyond the question of knowledge, if our authentic knowledge of the world is the result of the ability of the human mind to create ideas and concepts, the example that we have just given suggest that there is a fundamental difference in nature and epistemological status between science and the forms and the modes of representation of the real which came before it or run concurrent to it. But for some, more than a simple difference between “given” empirical knowledge, and

authentically scientific knowledge, there is a contradiction which means that the former is always an obstacle against the latter which can only form, therefore, by breaking radically with it.

That is, in fact, the central thesis of the epistemology of Gaston Bachelard, that particularly stresses the concept of “epistemological rupture” which involves both a conception of the formation of science, i.e. of the passage from prescientific to scientific and the evolution, or more precisely, of modalities of the progress of scientific knowledge, from its passage from one stage to another of its development.

From the prescientific to the scientific: the concept of “epistemological rupture” (Bachelard)

Man, as we have previously stated, did not wait for science to try to understand and explain the world. But for Bachelard, scientific knowledge does not prolong prescientific knowledge that he would gradually specify and examine in more detail. Instead, scientific knowledge sets itself apart radically and by nature. There is, rather, an ‘epistemological’ rupture between the two; a concept which, as we will later see, infers a discontinuist conception of scientific progress, but which also involves a certain relationship between science and the various immediate ways of understanding the real.

Let us begin by this second aspect of the problem which requires a detour by another central concept of Bachelard’s epistemology, that of “epistemological obstacle.” For Bachelard,

we should pose the problem of scientific knowledge in terms of obstacles. And it is not a matter of considering external obstacles, like the complexity and the elusiveness of phenomena, nor of incriminating the weakness of the senses and of the human mind: it is in the very act of knowing, intimately, that slowness and unrest appear through a sort of functional necessity. It is there that we show the causes of stagnation and even regression, it is there that we detect the causes of inertia that we will call epistemological obstacles and a light which always projects shadows somewhere. It is never immediate and full. The revelations of the real are always “recurrent”. Empirical thought is clear, “after the fact”, when the apparatus of reason has been focalized. By going back to a past of errors, we find truth in a true intellectual repentance. In fact, we understand against a previous knowledge, by destroying poorly constructed knowledge, in overcoming what, even in the mind, presents an obstacle to spiritualization (Bachelard 1967:13).

Among the “natural attitudes” which play this role of epistemological obstacle detrimental to science, Bachelard aligns himself with the mainstream of the rationalist tradition. He writes:

Science, in its need for completion as in its principle, absolutely goes against opinion. If it happens to legitimize the opinion on a point, it is for reasons other than those which constitute opinion; in such a way that opinion is always wrong *de jure*. Opinion “thinks” poorly, it does not think: it translates needs into knowledge. By designating objects by their usefulness, it does not allow itself to understand them. We can base nothing on opinion: we must first destroy it. It is the first obstacle to overcome. It would not suffice, for example, to correct it on particular points, by maintaining, as a sort of temporary morale, ordinary and temporary knowledge. The scientific mind forbids us to have an opinion on questions that we do not understand, on questions that we do not know how to formulate clearly. And no matter what is said, in scientific life problems do not arise on their own. It is precisely the “meaning of problem” which is the mark of the true scientific mind. For a scientific mind, all knowledge is a response to a question. If there has not been a question, there can be no scientific knowledge. Nothing goes without saying. Nothing is given. All is constructed (Bachelard 1967:14).

Just like opinion, but for a different reason, immediate intuition, what Bachelard calls “the first empirical influence” also distances us from science. He explains that this immediate, empirical way of understanding the real is incapable of getting to the heart of the matter, and gives us the phenomena only in their superficial, disparate aspect, in their disorder and diversity. Directly addressing empirical knowledge, Bachelard writes:

Prescientific thought does not hammer away at the study of a well-defined phenomenon. It is not looking for variation but variety. (...)

The research of variety drags the mind from one object to another, without method; the mind then only targets the extension of concepts; research on variation the variation follows a particular phenomenon; it tries to objectify all of its variables, to test the sensitivity of variables. It enriches the comprehension of the concept and prepares the mathematization of the experience (Bachelard 1967:29).

Another epistemological obstacle denounced by Bachelard lies in what he calls general knowledge. Writing on this type of knowledge, Bachelard says, “Nothing has slowed down scientific knowledge more than the false doctrine of the *general* which reigned from Aristotle down to Bacon (Bachelard 1967:29)”. Here, Bachelard is taking aim at the tendency – very close to empiricism, moreover – which

consists for the human mind in assembling facts to collect their similarities. He sees in this “distrust of all questions which would put forward resulting diversifications”, a “laziness of distinction,” and “signs of the fossilized concept.”

Bachelard sees a final obstacle in “substantialism,” this natural and particularly tenacious tendency of the human mind which comes down to believing that the impression that an object gives us comes from an intrinsic property, and hidden from it. He writes,

The substantialization of an immediate quality understood in a direct intuition no less hinders the later progress of scientific thought than the affirmation of a hidden or intimate quality, because such a substantialization gives rise to an explanation which is as brief as it is peremptory. It lacks a theoretical detour which requires the scientific mind to criticize the sensation. Indeed, for the scientific mind, any phenomenon is a moment of theoretical thought, a stage of discursive thought, a *prepared* result. It is produced rather than induced. The human mind cannot satisfy itself by purely and simply linking descriptive elements of a phenomenon to a substance, without any effort of hierarchy, without specific and detailed determination of relations to other objects (Bachelard 1967:102).

For Bachelard, these “epistemological obstacles,” far from being explained only by the complexity of the real or by deficiencies of our senses, find their origin in the very fact of knowing, and can, as a result, operate in the scholar’s mind, mislead his approach and as a result distort his comprehension of the real without his being conscious of it. Whence the need for a “psychoanalysis” of the scientific mind.

In a sense which does not totally correspond to the practice to which this concept refers in Freud’s work, the “psychoanalysis” recommended by Bachelard to free the scholar’s mind from beliefs sometimes inherited from the history of his discipline which can distort his understanding of the real or the interpretation of results of his research.

On the whole, with respect to the question of relationships between the prescientific and the scientific, Bachelard, by presenting the former as an obstacle to the latter, develops an approach of discontinuity of the formation of science which will also be expressed through the idea that he has of the progress of science. It is just as much through rupture that scientific thought is formed, as it is through rupture that it progresses. Scientific progress, he writes, “always manifests a rupture, perpetually ruptures, between common knowledge and scientific knowledge, as soon as we address an evolved science, a science which by the very fact of these ruptures bears the mark of this modernity” (Bachelard 1977:207).

On what do the “continuists” base what he considers to be an erroneous conception of scientific progress? A first reason lies in their postulate which is that of “the continuity of the history.” It is by virtue of such a postulate that they like to reflect on origins, they stay in the zone of the elementary nature of science.

Scientific progress was slow, very slow at first. The slower they are, the more continuous they seem. And as science slowly leaves the body of common knowledge, we believe that we have the definitive certainty of the continuity of common knowledge and scientific knowledge. All in all, here is the epistemological axiom posited by the continuists: since the beginnings are slow, progress is continuous. The philosopher goes no further. He believes that it is useless to live in new times, times when, precisely, scientific progress “explodes” from all sides, necessarily causing traditional epistemology to explode (Bachelard 1977:210).

Given that the “continuists” lose sight of the discontinuity of scientific progress, there is a second reason that Bachelard places under the tendency to attribute the credit for this progress to the “crowd of anonymous workers.” He explains:

We like to say that progress was “in the air” when the man of genius updated it. Then “atmospheres” and “influences” came into play. The further away we are from the facts, the easier it is to evoke “influences.” Influences are constantly evoked for the most remote origins. We have them cross over continents and centuries. But this concept of influence, so dear to the philosophical mind, hardly has meaning in the transmission of truths and discoveries in contemporary science (Bachelard 1977:212).

It is in the education tradition that Bachelard will find a third and final reason for the continuist error. It is natural that:

Since we believe in the continuity between common knowledge and scientific knowledge, we work at maintaining it, we feel obligated to strengthen it. We want to have the rudiments of scientific knowledge come out slowly and gently from good sense. We are reluctant to do violence to “common sense.” And in methods of elementary instruction, we put off for the sake of putting off the hours of aggressive initiations, we want to keep the tradition of “elementary” science, “easy” science; we make it our duty to have the student participate in the immobility of initial knowledge. We must, however, get to the point of “critiquing” elementary culture. Thus we enter the kingdom of difficult scientific culture (Bachelard 1977:212-213)

To finish up with Bachelard, we would point out that in the background of his discontinuist conception of the formation and progress of scientific knowledge, there is the idea that scientific knowledge, far from being able to lead to an absolute and definitive truth, can only ever be “approached,” and is thus always marked with uncertainty and indetermination. As we know, this idea is totally in agreement with the state of development of physics of his time, more specifically with the discovery of the “microphysical continent” and the arrival within scientific thought of relativist (Einstein), probabilistic (Bohr), and indeterminist (Heisenberg) tendencies which fundamentally question Laplacian determinism and the concept of science which results from it. In light of this idea, scientific development

appears as an endless approach of rectifications, reworkings and redevelopments of our concepts and theories, for ever more rigor and specificity in the understanding and formulation of the subject. Narrowly speaking, then, there can be no “exact science.”

In the way that we have just briefly summarized, Bachelard responds to the question of modalities of development of science, but it would be of interest to pursue the examination of the question through the debate which it sparked in the work of these two other emblematic figures of contemporary epistemology, Karl Popper and Thomas Kuhn.

The Popper-Kuhn debate: evolution or revolution of scientific knowledge?

Once formed, how does science move forward: by gradual accumulation, “growth,” of our knowledge, or rather by “revolutions”? As announced above, this second aspect of the problem of the formation of scientific knowledge has sparked passionate debates in epistemology the most notable of which divided Karl Popper and Thomas Kuhn, among others.

With respect to Popper, a first difference between his epistemology and that of Bachelard, concerns the “beginning of science.”

We will remember that while Bachelard sees the prescientific forms of knowledge as “epistemological obstacles,” Popper considers that science, philosophy and rational thought “should all start from good sense,” by which he means “all of the opinions and beliefs commonly accepted by men, i.e. all prior knowledge on which all of our knowledge is constructed.”

In fact, “good sense” in Popper’s work designates, “good critical sense.” Commenting on this concept, Malherbe writes:

For Popper, the starting point is always good sense, and the instrument of progress is the reconsideration of presuppositions promoted by good sense. It is by constantly transforming our prior knowledge that we form valid scientific hypotheses and are able to argue in a more and more critical way in philosophy (...) All growth of our knowledge is a series of conjectures and refutations. All our knowledge comes from errors that we have committed and our desire to no longer do so (...) (Malherbe 1979:131).

The progress of scientific knowledge itself takes place according to the same pattern; it is thus synonymous with the gradual perfecting of hypotheses and theories advanced to explain reality.

Seen from this perspective, all progress of knowledge lies, “in the improvement of existing knowledge modified in the hope of further approaching truth.” According to Popper, it always occurs in the following way:

A problem or a question (P_n) arises for a scholar and it must be solved by formulating a hypothesis (H_n). This hypothesis is then compared to experience in order to eliminate errors (EE). The modification of the hypothesis which results from this comparison engenders new problems (P_{n+1}) which we try to respond to with the help of a new hypothesis (H_{n+1}) and so on.

What is called science at a given moment is thus never anything but the set of hypotheses temporarily held to be true at that moment. This process is infinite insofar as the absolute certainty that some attach to the idea of science is only an ideal limit towards which research is headed, but which it can never reach.

We clearly see in this conception of development of scientific knowledge a reference to the biological model of the evolution of living beings as formulated in Darwin's theory, which has led to speaking of an "evolutionist epistemology." Like living beings, scientific theories are subjected to the requirements of adaptation and the struggle for life: those which survive or those which have overcome the "selection-elimination" test which here is called the trial and error method (which Popper also calls the critical method, that of refutation) which he, moreover, considers as the unique method of sciences.

Thus, for Popper, from the amoeba to Einstein, the growth of knowledge is always the same. Both can be wrong, even if, unlike the amoeba, Einstein is trying to consciously eliminate his errors and if, moreover, the amoeba dies from its errors whereas Einstein, thanks to their rational critique, can overcome them and move forward towards the truth.

In short, for Popper scientific progress lies in the improvement of existing knowledge modified with the objective of moving ever closer to the truth, which should not be understood in an absolute sense but only as a regulating idea allowing us to orient the quest for knowledge.

It is precisely this conception of development of science as a linear process of the infinite accumulation of knowledge that Kuhn rejects by contrasting it with that of "scientific revolution" of which we can only fully understand the meaning and the reach from the perspective of the concept of "paradigm" which underlies it. What do we mean by "paradigm"? It is true that the concept is fairly ambiguous in the work of Kuhn himself. Sometimes, it refers to "scientific works universally recognized which provide problems and model solutions to a community of practitioners during a certain time," works on which "traditions of particular and coherent scientific research" are based. Other times, we are referring to a set of "diverse theories having a high frequency and a more or less standardized content," that we find in "texts, classes and laboratory exercises." It acts as a "narrow network of conceptual, theoretical, instrumental and methodological postulates," or as "an implicit set of overlapping theoretical and methodological beliefs which make selection, assessment and critique possible."

With the support of these various definitions – and the list is not exhaustive – given by Kuhn, we could see in the paradigm what, at a particular stage of the development of science, allows the community to communicate and agree on the admissibility of the formulation of questions and responses to provide for them, i.e. on what it can recognize as scientific because it is in accordance with principles, rules and procedures, the validity of which is consensually accepted within it. By choosing this concept of paradigm, writes Kuhn,

I want to suggest that some recognized examples of real scientific work – examples which include laws, theories, applications and experimental systems – provide models which give rise to particular and coherent traditions of scientific research. (...) It is the study of the paradigm which, mainly prepares the student to become a member of a particular scientific community with which he will later work. As he joins here with men who have drawn their bases of knowledge from the same concrete models, his work will rarely lead him to disagree with them on fundamental points. Men whose research is based on the same paradigm adhere to the same norms in scientific practice (Kuhn 2008:30).

The paradigm thus understood refers to the conservative, traditionalist side of science, i.e. to what Kuhn calls “normal science” which is spread through professional training of researchers, in textbooks and in well thought of and approved scientific reviews. It is also according to the dominant paradigm that ranks, privileges and academic status are distributed with their social or even material implications. An essential characteristic of the paradigm is its “incommensurability” which closes it in on itself and makes dialogue from another paradigm impossible.

As long as the facts to be explained fit without difficulty into the “conceptual boxes” of paradigms, it is “normal science” which continues to function. Scientific activity is then reduced to increasing knowledge of these facts and, more precisely, as Kuhn writes, their correspondence to predictions of the paradigm all the while refining the formulation of this paradigm itself. Scientific activity is reduced here to the resolution of simple “enigmas.” “Novelties” can probably appear in normal science, but they are not born out of nothing. As Kuhn writes:

They emerge from ancient theories and inside a matrix of ancient beliefs concerning phenomena that the world contains and at the same times does not contain. Normally, these novelties are much too esoteric and abstract to be observed by a man who has not received serious scientific training (...). On the contrary, starting from research undertaken as part of his doctoral dissertation, the practitioner of a mature science continues to work in regions that the paradigms inherited from his education and the research of his contemporaries seems to be able to successfully analyze. In other words, he

tries to elucidate the topographic details of a map of which he knows the major lines in advance; and he hopes – if he has a depth of view allowing him to identify the nature of his field – to one day devote himself to a problem which will give rise to the unexpected (...). In the mature sciences, the precondition for most discoveries and all new theories is not ignorance but the recognition of a failure in knowledge and existing beliefs (Kuhn 1980:287-288).

It may happen, however, that scientific activity, in its normal course, runs into an “anomaly,” i.e. facts of phenomena which keep at bay the explanatory power of science on the basis of the paradigm in effect, particularly by questioning the effectiveness of concepts, theories and method admitted up until that point by the scientific community. When a paradigm shifts, not only are “the source of methods, fields of problems, and types of solutions accepted by a whole mature scientific community at the time” changed, but also the necessity of the “redefinition of the corresponding science,” just as “the criteria by which we distinguish a real scientific solution from a metaphysical speculation” change. Kuhn explains that the tradition of normal science which emerges from a scientific revolution “is not only incompatible with what was happening up until then but also incommensurable.” The incommensurability of paradigms makes the derivation of a new theory from a previous theory impossible.

The impossibility for “normal science” to integrate these anomalies into its framework of intelligibility then plunges the scientific community into a state of “crisis” which cannot itself be overcome by a “scientific revolution.”

Scientific revolutions which thus appear as “non-cumulative episodes of development in which an older paradigm is replaced, in its totality or in part, by a new incompatible paradigm,” (Kuhn1983:131) does not only intervene in the epistemological sphere. They also have a sociological impact, if not political as well. Kuhn points out, moreover, that it is completely on purpose that he uses the word revolution which normally belongs to the political register. As in politics, beyond a simple paradigm shift, all the institutions, rules and all criteria of recognition which are shaken within the scientific community are reconsidered from the feeling that they have ceased to be able to function in a satisfactory way. Thus, it is not only new theories which appear, but also new scientific authorities, new criteria of cooptation and recognition within the community, new educational texts and programs, with all that they include by way of reconsidered acquired knowledge, interests and sometimes purely material advantages which were obtained on the basis of the overturned paradigm.

We then understand that, always following the example of political revolutions, the supporters of the old paradigm put up a good fight and we sometimes have to resort to methods of mass persuasion, or even violence³.

It is all of these considerations which constitute and clarify the way in which Kuhn conceives of scientific progress. We can clearly see the difference between his conception and that of Popper in the following:

Contrary to dominant opinion, most new discoveries and scientific theories are not simple additions to the existing reserves of scientific knowledge. In order to assimilate them, the scholar should normally rearrange the intellectual and technical equipment on which he based them, all the while pushing aside some parts of his belief system and previous practices and discovering meanings and new relations between other elements. Assimilation once again involves the reassessment and reorganization of the old, discovery anew involves the reassessment and reorganization of the old; discovery and invention in the sciences are intrinsically revolutionary. Therefore, they require precisely this flexibility and open-mindedness which characterize, or even define the divergent thinker. Thus, we should admit the need for these characteristics going forward. If these qualities were not the prerogative of numerous scientific researchers, there would be no scientific revolutions or very few scientific revolutions (Kuhn 1983:131).

The Scientific Approach: Questioning Logic and Experimentation

The scientific approach includes two fundamental aspects, namely questioning and experimentation. The first is more characteristic of sciences like mathematics and logic, although, as we will later see, it cannot be lacking in any science (don't we speak of "experimental questioning" in the natural sciences?), whereas the second, first considered the prerogative of the natural sciences, has a tendency to be established as a criterion par excellence of any scientific approach.

Logical questioning

In defining the criteria of scientific thought above, we spoke of the concept of rationality. One of the definitions that we have given for it, is that which is in accordance with the principles and requirements of logical thought, such as they were formulated by Aristotle.

In science, the principles of logical reasoning function not so much as a means of knowledge production as a way to organize thought to make possible agreement on what can be held to be true or false. Yet, these would seem to be a minimum when we are dealing with coming to conclusions which we expect – unlike those in metaphysics which are the subject of endless discussions – will achieve agreement. They are both the formal condition and guarantee of the possibility of a universal knowledge.

Classical logic is based, as we know, on three principles which are: the principle of identity (a thing is always identical to itself, its "same old self"), the principle

of the excluded third party (between A and not A, there is not a third possibility) and the principle of contradiction, which we also sometimes call the principle of non-contradiction (A and not A cannot exist at the same time and under the same relationship, the two absolutely exclude each other).

No question of our going into detail here in the statement of these principles, and even less of our dwelling on the controversies that they can arouse. We would simply note that “without these principles, the very exercise of thought appears impossible,” stresses A. Virieux-Reymond, who considers that they orient the activity of all thought which claims to be rigorous and rational. He writes the following on this subject:

For people able to be mistaken as we are, the three fundamental principles which direct our activity of judging in its affirmations and its negations are mutually involved and they intervene directly or indirectly in all approaches of reflective thought. Indeed, as unstable as the data are that we want to use to create science, it is necessary (for fear of not being understood either by others or even ourselves) that once a term is defined a certain way, another meaning not be given to it during the exposé, without warning the reader or listener, whence the principle of identity; it is also necessary that a term and its negation not be attributed at the same time in the same relationship and the same point of view to the same subject (whence the principle of contradiction), nor that a third possibility intervene between A and not-A – which we repeat, should not be identified with not contrary to A: it is what is not A, what is other than it without its necessarily being diametrically opposed as the contrary (excluded third) : if this were the case, the negation would lose the valuable apagogic power that it has since a third possibility could slip in between A and not-A and the negative thought would become unusable: the negative statement denounces our errors by inviting us to search for the true judgment other than in the denied fact. Without the two latter principles, questioning by the absurd becomes unusable (Virieux-Reymond 1966:59).

Logical questioning is not, however, sufficient in and of itself to confer a scientific value on knowledge. The type of truth that it enables us to obtain, i.e. the formal consistency of the structure of statements such as those we can discover in the syllogism, is not necessarily synonymous with appropriateness with the real. By way of illustration here, we can take the famous syllogism of Socrates, “All men are mortal; Socrates is a man, thus Socrates is mortal.”

From the perspective of formal validity, this proposition is absolutely indisputable as its conclusion naturally follows from its premises. But if we keep its formal structure all the while changing the terms, we can observe that it can, however, be false in the facts without losing this formal validity. For example:

“All students in the social sciences are intelligent; Charles is a student in the social sciences, thus Charles is intelligent.” This statement, although it has the same form as the first, can be true or false, depending on the result of its comparison with reality – i.e. with Charles and his real academic performance.

We should, however, note that deduction, which is the type of logical questioning which we have mentioned here, is not the only sort. There is also induction which, because at the very least it escapes the critique that we have just made of the deductive approach, in that it starts from observation, is presented by some as the true approach to science. Induction, we should recall, is the approach which consists in starting from the largest possible set of observed facts to draw a general conclusion from them. It thus presupposes the fundamental postulate of empiricism, according to which all of our authentic knowledge derives from experience. We will have the chance to return to a more in-depth look at this form of questioning when we address the experimental method.

In the mean time, we can already note why, no more than the purely deductive approach, induction does not constitute the noble pathway of scientific knowledge. We can criticize it for three limitations.

The first limitation of induction lies in the quality of knowledge it provides us. This knowledge can never be certain, as successfully stressed by Popper, one of the most resolute adversaries of inductive knowledge. Given the inventory of particular facts which can never be exhaustive and observations not infinitely repeatable, it is always to be feared that a new observation will refute the preceding ones. It is not, Popper tells us, because up until now no one has seen a black swan that the statement “all swans are white” is an absolute certainty. Indeed, from the point of view of logic, nothing stops us from thinking that a swan could be black or affirming that a black swan can never exist. Furthermore, and this is the second limitation of induction, I can only understand the statement that “all swans are white” if I know what a swan is, which, in accordance with the postulate of induction, is only possible following observations. This is why induction is in itself a sort of vicious circle. Finally, induction is related to immediate experience, to what Bachelard called “initial knowledge” which is incapable of criticizing itself and rising to the level of the development of concepts and the establishment of laws.⁴

Given all of these reasons, it is then important to successfully distinguish the inductive approach, strictly speaking, from the experimental method which we also call experimental questioning.

Indeed, if the two are similar in that they confer a significant place in scientific knowledge on facts, they differ from each other in two fundamental aspects, namely, the way in which they each understand the concepts of “facts” and “observation” and also the way in which they posit the relationship between theory and experience which is, moreover, related to the first aspect.

The experimental method

The importance of this method which has allowed science to progress considerably is such that it is worth our while to spend a bit more time looking at it. We should stress both its role and its originality in the history of scientific thought. Carnap writes:

One of the principle characteristics which distinguish modern science from that of the previous periods lies in the very particular significance of what we call the “experimental method.” All empirical knowledge is, at the end of the day, based on observations but they can be obtained in two ways which have an essential difference. When we use the non-experimental method, we play a passive role. We content ourselves with looking at stars or flowers, noticing the similarities and differences, and looking for regularities which can be expressed as laws. On the contrary, in the method of observing which we call experimental, we take an active role. Instead of waiting for nature to give us situations to observe, we try to create them. In other words, we engage in experimentation (Carnap 1973:47).

In the presentation normally made of it, the experimental method – first and foremost that of the natural sciences before being considered as the single method that all the sciences should apply to deserve their status as full-fledged science – includes three necessary moments: observation, hypothesis and verification. We find a detailed exposé in the work of Claude Bernard who was the main theoretician of this (Bernard 1966).

Observation

Unlike the logician, or even the mathematician, the scholar in the natural sciences starts with facts. He observes phenomena such as they unfold before his eyes in their diversity and apparent disorder to try to understand them and to render an account of them in a satisfactory way for the mind in the way which they appear, develop, link together over time and space, and then disappear. But the idea that the scholar starts with the facts is worth specifying for fear of being false.

The first detail to add is that the scientific fact, unlike the “gross” fact such as we see in purely empirical observation, is a constructed fact rather than a given. In other words, it is the particular interest that it assumes for the scholar, the fact that for one reason or another it creates a problem with respect to his own scientific theories or acquired knowledge of existing science, which pushes us to be interested in one fact more than another, to attach more importance to one fact over another. In other words, not only is the scientific fact always a selected fact (thus all facts do not have the same value for all scholars or even for the same scholar), but also it is immediately imbued – if we might express it this way – with theory. It is with respect to this point of view that Claude Bernard says that “science always starts from a preconceived idea.”⁵

Unlike the passive or even blind look that the common mortal casts on the sequence of phenomena, that of the scholar is directed by his own scientific questioning. Of all the facts which appear to the eye of the scholar the only ones worthy of interest are those of which the existence calls out in one way or another to the researcher. It is in this way that Bachelard speaks of “polemic fact.”

The hypothesis

The facts observed by the scholar do not “speak” of themselves; their intelligibility is not immediately obvious. This is why, once the researcher has defined a field of research, he should be able to propose a temporary explanation which is, in the etymological sense of the word, the formulation of a hypothesis. It is at this stage that we probably most see the creative imagination of the scholar in the production of knowledge. A good hypothesis should be necessary, plausible, strong, fecund, and open.

Necessity: When the arsenal of laws and scientific theories available is sufficient for making a fact or a phenomenon intelligible, we do not need to try to “reinvent the wheel.” The hypothesis should thus imperatively be able to – if it is confirmed – allow science to progress, either by reworking or challenging its former laws and theories and, at any rate, by enriching, deepening and broadening.

Plausibility: Even if this is only the test of the verification that a hypothesis should be accepted or refuted (for good), its admissibility still requires a prior condition. For this reason, it has to be plausible, i.e. we cannot reject it immediately on the basis of a rigorous logical argument or confrontation with laws, principles and scientific theories duly established.

Force: When the scholar is looking for hypotheses, a number of them can appear to him and this plurality is in itself an excellent thing to the point that he should not deprive himself of working on the maximum possible number of hypotheses. But all hypotheses are not the same. First, we must always be aware of the most tempting hypothesis because it can mislead the scholar, and often it does so. We should also be aware of the most practical hypothesis, the most comfortable for the mind because, upon analysis, it can prove to be fragile. A good hypothesis is thus the one which was temporarily chosen at the end of a Darwinian process of natural selection, i.e. the fight for life.

A hypothesis can be considered strong when it resists all of the demanding attempts to refute it. It is only when it has survived the test of rational critique better than all the other rival hypotheses, like the wrestler who covets the champion’s flag in the ring, that it can be chosen among all. But the value that it derives from its competition with other credible hypotheses is temporary.

Productivity: A productive hypothesis is one which opens up real prospects for the research by allowing the researcher to make progress and possibly to reach a conclusive scientific result. In other words, it should lead somewhere, and thus have heuristic value.

Opening: This last characteristic extends and completes, as it were, the one that we have just discussed. Indeed, in order not to block research, not to neglect or overlook any prospects which might be suggested to it, in order not to compromise in advance any possibility, the hypothesis should be open to the numerous logical or experimental reasons that the scholar might have to redevelop, rework or reformulate it, or simply reject it for another; i.e. the scholar should not cling to his hypotheses no matter what as if it were a question of life or death for him.

This means that research is a permanent risk: the researcher always runs the risk of being rejected at his starting point, contradicted in his initial intuition and hypotheses. But even when it fulfills all the criteria that we have just outlined, the hypothesis can only be accepted after having been successfully submitted to the test of verification.

Verification

The determinative importance given to verification is a logical result of the inductivist postulate which the conception of science copied from the model of the natural sciences is based on. By inductivist postulate, we mean the idea that from a given set of particular identical and corroborating facts, we can derive a general law valid for each of these particular facts.

And the objective of verification is to show that the hypotheses formulated from a set of facts can be validated by the successful comparison with other facts of the same type. Verification, in the sense that we mean here, is also a result of the fact that the scholar accepts nothing which has not been subject to proof. It can, depending on the sciences, work in different ways. The physicist and chemist, for example, proceed by trying to reconstitute the elements which the hypothesis is based in the laboratory to see if it renders a conclusive account or not. In another natural science, biology, verification is not always done in the laboratory, and sometimes presents difficulties for ethical, moral, or religious reasons for following the approach used in the sciences of inorganic matter. This impossibility of experimental verification is not only valid for biology. Carnap also gives the example of astronomy.

In astronomy for example, we cannot deviate a planet from its orbit to see what will happen. The objects studied by astronomy are out of reach; we can only observe and describe them. Sometimes, the astronomer is able to reproduce in the laboratory similar conditions to those that we find, for

example, on the surface of the sun or moon, and observe what happens in these conditions. But this is not a true astronomical experiment. It is a physics experiment which is of interest for astronomy (Carnap 1973:47).

Furthermore, we must understand that, even subjected to the most honest, scrupulous verification possible, a verified theory is nonetheless still a temporary theory. We must always bear in mind that the possibility of the discovery of new facts, or technological innovations allowing us to refine understanding of the real, requires that we go back to the drawing board with such a theory, either to restrict its field of validity and its explanatory power, or to simply reject it for a new theory more likely to throw light on this field.

Finally, verification, such as practised in the experimental method, and contrary to what the narrowly positivist conception of science thinks which confuses it with an essentially quantitative “thing-centered” approach” of accumulation of “facts,” of “data” and showing figures which speak for themselves, involves a constant back and forth between theory and experiment, the first highlighting the second, and the latter validating the former. It is this dialectical relationship between theory and experience in all authentic scientific approach that Claude Bernard summarizes in these terms allowing us, in the process, to come back to the difference between experiment in the popular meaning of the term and scientific experimentation:

The complete scholar is the one who embraces theory and experimental practice: 1) he observes a fact; 2) an idea about this fact is hatched in his mind; 3) in view of this idea, he reasons, institutes an experiment, imagines and realizes the material conditions of it; 4) from this experiment new phenomena result which must be observed and so on. The scholar's mind is in a way always situated between two observations: one which serves as a starting point for questioning, and the other which serves as a conclusion (Bernard 1966:55).

Claude Bernard continues that, in experimental questioning, there is such a tangle between what results from observation and what belongs to the experiments that it would be impossible and, furthermore, useless to want to analyze each of these two terms in their inextricable mixture. We just need to remember that the *a priori* idea, or better yet the hypothesis, is the stimulus of the experiment, and that we should go at it freely, provided that we observe the results of the experiment rigorously and completely. If the hypothesis is not verified and disappears, the facts that it will have helped us to find will nevertheless remain established as immovable materials of science (Bernard 1966:27).

Scientism as “Ideology” of Science

The principles and the criteria of scientific character outlined above, principles that no scholar can violate without losing his title to be considered a scholar, have often been diverted and denatured by a attitude which claims to be representative of science, all the while being its pure and simple perversion: scientism.

Scientism is an “ideology” in the exact sense that Karl Mannheim, like Marx, gives this word. It is: a vision which denatures real scientific practice, and gives a “false awareness” of what scientific practice really is. Thus we should not, as we often do in everyday speech, confuse the adjectives “scientific” and “scientistic.” The authors of a collective work on epistemology write that:

Scientism is the scientific belief according to which the results of sciences are placed above all philosophical critique. This paradoxical act of faith which, in the name of reason, denies reason, leads particularly to the assertion that philosophical questions, indeed questions from the various social sciences, can be resolved only through the natural sciences. It is an extreme form of reductionism, or a corruption of the methodological approach which is established as an absolute in the name of a supposed rationalism which is precisely the opposite of well understood rationalism, i.e. of an open rationalism (Arago 2006:158).⁶

One of the most important scientistic principles lies in the idea that knowledge par excellence is scientific knowledge. This idea has as its corollary the disqualification of all other forms of knowledge as if there could be no other truth but objective truth. Now, let us take the case of the believer, not the false believer, but the authentic believer: the subject of his faith has the same force of evidence for him as a mathematical or physical truth. Obviously, a “positive” mind (in the meaning that Auguste Comte gives the word) could, in the name of the scientific necessity of proof, reject this evidence, but it could not found this refusal on the principle that he invokes. Indeed, if it is true that it is impossible, for example, to prove the existence of God on empirical bases, it is just as true that we cannot found the refusal of this existence on the same empirical bases. The reason for this is that the observable depends on the state of our senses and their abilities to faithfully record the data of the external world and represented them to us as such. Yet, everyone knows that we can believe that we saw what we have never seen, like the traveler who, lost in the middle of the desert, thirsty and hungry, sees oases on the horizon with their fresh water source. Mirages of this sort, of which we could provide numerous examples, are a part of objective reasons that we have to distrust our senses. Didn’t Engels say on this subject that empiricism is the most direct path leading to mysticism?

Another objection is that something can be real without being visible. Microbes, obviously, existed before the microscope. Their reality was as indisputable as when it became so after the discovery of this wonderful instrument which today allows us access to the unfathomable depths of the infinitely small. Some realities, including non-existence, cannot be established on the basis of observation but that the human mind can indeed see, would run the risk of simply seeing their existence denied by an empirical conception of reality which would reduce it to observability.

Another aspect of scientism which could provide an occasion for critique lies in its objectivism. It is an aspect which, along with others, has particularly attracted the attention of Popper. It might be all the more interesting for us to stop and look at this aspect, given that Popper's critique of it is based on a double conception of the status of scientific theories and the method of their production which creates the originality of his epistemology.

The absolutization of the concept of objectivity in science first proceeds from the idea that certainty is possible in science, which is for Popper an illusion which results, according to him, from what he calls "the old ideal of the episteme, the ideal of an absolutely and demonstrable knowledge (Popper 1984:287)" which, he continues, "proved to be an idol." The homage rendered to this idiom "not only represses the audacity of our questions but, moreover, compromises the rigor and honesty of our tests" so true is it that "what makes the man of science is not the possession of irrefutable truths, but the obstinate and audaciously critical search for the truth."

Scientific certainty is thus a simple illusion and all our scientific theories are only simple "conjectures," which ruins "*the absolute authority of science*" (Popper 1979:190), by virtue of this "fallibility" which is the very mark of our condition of man, without meaning the renunciation of all idea of truth, insofar as the idea that "error is possible and that the search for certainty, or even a high probability is vain does not mean that we are wrong to seek the truth (Popper)." Indeed, this quest presupposes that "the truth is our goal."

In any event, continues Popper – and it is through his criticism of scientism, of the idea of truth which is inherent to it and of the method on which truth is based – we emerge upon what Popper considers the approach par excellence. This approach is diametrically opposed to the "verificationism" of the inductivist experimental method, such as defined above following the example of Claude Bernard, and which will be taken up again in their way by Wittgenstein and the theoreticians of the Circle of Vienna.⁷

For Popper, "we can never empirically justify (...) the claim that a scientific theory is true" (Popper 1982:23). This is the chance for him to denounce the conception of the scientific method, which he calls "methodological naturalism,"

a conception which, according to him, is often “reasonably believed,” including by specialists in the “social sciences.” This methodological naturalism, he defends,

sets requirements of the sort: begin by observations and measures, including, for example, statistical research; then move on to induction and generalizations and the formulation of theories. In this way, you will approach the ideal of scientific objectivity, insofar as this is possible in the field of the social sciences. You should be aware of the fact that objectivity is much more difficult to attain in the social sciences than in the natural sciences (insofar as it can ever be attained). Because objectivity means the absence of a value judgment (...) and that which the social sciences practices can only – in very rare cases – emancipate itself from the values of the social class to which it belongs to achieve a certain degree of neutrality and objectivity (Popper 1979:83).

In reality, for Popper, this is not the right scientific method, either for the social sciences or for the natural sciences, nor is there one method for the former and another for the latter. There is a single, unique scientific method which, for Popper, is not the experimental method but the hypothetical-deductive method, or “trial and error,” (he speaks again of “critical method”), which Popper summaries in this way:

The theoretician will do his best to detect all false theory in the set of rivals not refuted; he will try to “catch” them, i.e. for each of them, he will try to imagine cases or situations where the theory will probably fail if it is false. He will then try to fine-tune rigorous tests, and crucial experimental situations (Popper 1987:76).

In light of this conception of the scientific method, scientific objectivity depends “uniquely and exclusively on the critical tradition which, despite resistance, often makes possible the critique of a prevailing dogma (Popper1987:89)” In other words, it

is not a question of the individual, with the exception of interesting men of science, but a social question which results from their mutual critique, of the friendly division of work – or hostile between scientists – of their collaboration as much as their rivalry. It thus partially depends on a series of social and political conditions which make this critique possible ...

No matter which science is considered, this objectivity, Popper continues, can only be explained by social concepts such as competition (both of men of science among each other and different school of thought), tradition (namely, the critical tradition) the social institution (for example, publications of various competing reviews by competing editors), free discussion, including politically tolerated discussion (Popper 1987).

A last aspect of scientistic ideology which we think deserves to be noted and which is, furthermore, connected with what we have called objectivism, lies in the ignorance or refusal to consider the idea that, no matter how scrupulous and vigilant it attempts to be, the activity of the researcher necessarily suffers from a certain number of extrascientific factors of which he is not always conscious, or that he believes to have eliminated by his approach. But without his knowing, they interfere with his research and at the very least call on him to put the results in perspective. This is what we call the “unthoughts” of scientific work.

Such “unthoughts,” that the scholar should be able to recognize at the risk of deluding himself about the value of his work, which means that he agrees to submit to a certain “psychoanalysis” as recommended by Bachelard, can be of a number of sorts – philosophical, religious, cultural, ideological, moral and even epistemological.

We will illustrate our statements with the help of several examples.

Philosophical unthoughts

The attitude of the scholar can be inferred with or even trapped by implicit philosophical positions which can be a barrier that distance him from the knowledge of reality. There are no scholars, for example, who do not implicitly adhere to the philosophical thesis that the real is knowable by the mind, even if there can be differences of opinion on the degree of knowability of this real, which also comes under philosophy, because rejecting such an idea, is to disqualify *a priori* the ambition of knowing the scientific project itself. However, the scholar can have the conviction not only that what he is doing has nothing to do with philosophy but even that it is only fully and authentically in philosophy insofar as he knows how to completely rid himself of philosophy. Yet, we know that there was a whole current of contemporary epistemology, namely logical empiricism that is also called logical positivism, from the Circle of Vienna whose program was precisely to eliminate “metaphysics” (understood here as traditional philosophy).

With respect to this philosophical unthought, Althusser spoke of the “spontaneous philosophy of scholars,” a philosophy that they are able to more or less repress when science is developing normally, but which resurfaces when their science enters into a “crisis,” which leads them to “throw their philosophical fit.”

Religious unthoughts

For some, not only does religion have nothing to do with science, considered by some as its absolute antithesis, but it is also considered the greatest enemy of science, the greatest obstacle to the development of scientific thought. The opposition of the two would be that of faith and reason, incompatible in their

respective natures in each of them. In other words, to be scientific would be to be rational to the end and radically eradicate from science everything that was even remotely related to faith. However, things are not so easy.

First, it is frequent to see scholars very reticent to commit to themes or directions of research, not always because such directions could not advance science, but rather because unconsciously for them, taking such directions “would hurt” their religious convictions in one way or another, without the reason for their reticence being totally apparent to them, or they refuse the interpretation of one scientific result or another by invoking scientific arguments which sometimes only hide religious convictions. Nothing illustrates this idea better than the often passionate debates that the theses of Darwinian evolutionism on the question of the origin of man provoke between supporters of the opinion according to which man’s appearance is the result of a long evolution of the species, and those who believe that he was created by God and in his image. Obviously, it is rare to hear scholars who reject the first thesis say that they do so in the name of religion but it is instead arguments – scientific to their mind – with which they challenge it. We can also say, in the same vein, that the debate which during Galileo’s time, pitting supporters in the scientific community of heliocentrism against those of geocentrism which was also the official doctrine of the Church, had a religious background which was not always obvious for supporters of this second idea.

Ideological and political unthoughts

Here we find here a perfect illustration in the distinction which was legal tender during the period of Stalin between “bourgeois science” and “proletarian science.” This distinction resting on the postulate that the objectivity proclaimed by scholars and the presumed neutrality of science were only subterfuges aimed at masking the fact that all truth, even scientific, has a class character, and thus that truth in science was only a matter of the class wars. One of the consequences of this idea – which fortunately did not last long, but unfortunately had the time to create serious damage in the evolution of science in the ex-Soviet Union – was a delay in the development of genetics dedicated to genomics for having postulated the existence of the gene. The idea of a reality being able to escape from the principle of becoming was considered contrary to the ideological foundation of the state and, for this reason, deserving of the most severe sanction.

This example also shows how, in the name of a certain political or ideological conformity, the scientific community can end up defending theses which, in reality, have nothing to do with science.

It is also this ideological unthought that Lenin wanted to bring to light in his famous work entitled, *Materialism and Empiriocriticism*. All of Lenin’s effort in this work was to show that the interpretation by a good part of “bourgeois” scholars

of the time, of the discovery of atomic energy as proof that “matter disappears” had, beyond a simple scientific debate, a hidden ideological meaning. It was, according to Lenin, a way for these scholars to refute the existence of matter, to undermine the foundation of materialism in the name of the idea that everything is in the end definitive energy, i.e. to show that only idealism is in accordance with science. For Lenin, it was thus no more and no less Marxist ideology and its political and social plan which was targeted in the end.⁸

Cultural unthoughts

The scholar is first and above all a man, i.e. a social being, member of a human community whose cultural values deeply impregnate existence. From this point of view, and no matter what effort he may make and what desire he may show to want to distance himself from the opinion and collective representations of his community, he cannot totally detach himself from it. This cultural context always acts in a particular way on him. This is even more true in the social sciences, where it is more obvious that the research subjects are culturally connoted, over-determined. If this cultural determination does not appear explicitly through the results themselves, it can be felt very clearly first through the interpretation of these results. We can more easily find an illustration of this idea in the social sciences where, a belief or an explanation rejected as false or absurd through a given cultural prism can have a completely rational basis when we understand it in light of the concrete cultural context which is particular to him.

The cultural determination of science can also be expressed through the very idea that we have of science. We know that Europe has lived for a long time with the idea that there can be no science other than western science, all knowledge being produced then often disdainfully disqualified as part of the murky universe of myths and superstitions peculiar to “prelogical mentality”, thus prescientific, if not antiscientific. Thus in Africa, we are struggling today, through debates on the status of our “endogenous knowledge,” for a “decolonization” of the concept of science which enables this knowledge to be recognized as automatically having a scientific status. However, there are a number of scholars, not only Western but even African, who, naively or deliberately, have transformed themselves into the apostles of a universalist conception of the idea of science which is only, in reality, the expression of a cultural prejudice which proceeds while being concealed. The supporters of this attitude do not even wonder how the people to whom they refuse this science could have been able to live through the ages if they had not been able to have sufficiently valid knowledge of their natural environment, their natural milieu to allow them to resolve the problems that it presented to them.

The reference to these unthoughts of science is not at all aimed at disqualifying the ambition of the scholar to produce knowledge which has chances of being rendered universal. It is to push science towards more humility and scholars to

more modesty and moderation in the way in which they understand and present the value of their activity. We must arm the scholar with the vigilance necessary in view of an idea which presents science as exempt from all forms of prejudice, whereas the mind of the researcher is always full of prejudice, but also to understand that the “reality” behind which he has the tendency to take shelter to defend his theories, far from being a “given,” is quite often only a simple philosophical, political, ideological, religious or cultural construct. Besides, these considerations are of great importance for the epistemological status of the social sciences.

Some specific problems of the epistemology of social sciences

If we stick to the criteria of the scientific character that we have laid out above, the epistemological status of social sciences immediately creates a problem. The problematic character of this status can be basically found in three points which specify, as it were, social sciences.

- a) The first concerns the nature of their subject which can be distinguished from that of the natural sciences in many respects. Indeed, the subject of these sciences is not at all identically reproducible, lends itself with difficulty to generalization, and cannot think of itself in isolation without being evaded, not to mention that it is constituted of everything which makes up the daily life of man, namely an always particular mode of insertion in reality, passions, beliefs, values, prejudices normally rejected as so many elements prejudicial to scientific objectivity.
- b) The second concerns the relationship that the researcher in the social sciences maintains with his subject. Unlike the scholar in the natural sciences who can and should treat his subject as being foreign to him, and keep it at a distance which is the very condition of his objectivity, the scholar in social science is, as a social being, involved in his subject. He is, in other words, subject and object of his own research. Knowledge in social sciences is thus always imbued in one way or another with a certain subjectivity.
- c) The third difference between the natural sciences and the social sciences lies in the objectivity of knowledge in the social sciences.

In going on these differences, among other things, we ended up refusing these sciences the claim of sufficient objectivity to be able to deserve to be considered as authentically scientific. Hence the need, before going further, to return to the concept of objectivity to denounce what we might call the objectivist illusion since, if we take a close look, it is in the name of such an illusion that people try to epistemologically disqualify the social sciences.

Objectivity of science and absolute neutrality of the scholar

In setting out the criteria of science as it is generally understood, we have stressed objectivity. This does not mean that the concept of science is questionable, but rather, the way in which this objectivity can be understood. Yet, from this point of view, what we have designated as an erroneous comprehension of science, namely scientism, includes a corollary just as prejudicial to science. This corollary is objectivism.

By objectivism, we mean the conception according to which science fully reproduces reality for us, such as it presents itself to us with no extraneous additions, to the point where our knowledge of this reality is nothing but the faithful and loyal expression of continuous and necessary relationships which exist between phenomena. This idea, linked to that of the absolute neutrality of the scholar, proves to be nothing but an illusion, a pure and simple myth. Popper writes,

We cannot strip a man of science of his partiality without stripping him of his humanity as a result. Moreover, we cannot forbid or destroy his value judgments without destroying him both as man and as a man of science. Our motives and our purely scientific ideals, such as the ideal of the pure search for the truth, are deeply anchored in extra-scientific values, particularly religious ones. The “objective” man of science, “detached from all values,” is not the ideal man of science. Nothing occurs without passion, even pure science. The expression “love of truth” is not a pure metaphor (Popper 1987:89).

But an even stronger reason to relativize scientific objectivity lies in the fact that science is a human work. As a man, the scholar has an existence limited in time and space. He is a finite and narrow-minded being. This is why no matter what his concern for fidelity and loyalty with respect to his subject, he can only understand reality and represent it from the perspective of this double limitation in time and space. Absolutely objective knowledge, thus capable of reproducing reality in all of its depth and complexity, is only possible if the scholar puts himself in the place of an omniscient and omnipotent God, capable of encompassing the infinity of the real, the infinity of relations which constitute it in a single look.

In other words, scientific knowledge can and should only be taken seriously when it breaks with the illusion of absolute objectivity; it knows and recognizes that it is only a simple window, opening out onto the immensity of the real, a simple “perspective” which, because of this immensity, should always and necessarily be further extended.

If absolute objectivity does not exist and if the absolute neutrality of the scholar does not exist either, if all knowledge necessarily bears the mark of human finitude, the objection of wanting to epistemologically disqualify the social sciences under the pretext that they are incapable of objectivity and absolute neutrality

becomes inadmissible. Because on the basis of the same objection, we could refuse this status to any science. Nonetheless, the social sciences bear a specific scientific character, to be understood here in the sense that they are capable of implementing a method, approaches and procedures enabling them to construct in their specific field a completely respectable objectivity, although different from that proposed by the natural sciences.

Epistemological impact of the complex relationship between the scholar and his subject

Another argument which has often been advanced to dispute the full-fledged scientific character of the social science is based on the nature of the relationship which exists in these sciences between the researcher and his field of research. The natural sciences imply and require a clear differentiation between the research subject, between the scholar and the field which he wants to render an account of scientifically. In the social sciences, the researcher is both subject and object of his research. He is “subject,” i.e. researcher and thus required as such to respect conditions, principles, rules and theoretical and methodological imperatives of all good scientific research. He is, nonetheless, a man, and thus an integral part of his subject of research.

Obviously, his attitude with respect to the values of the society of which he is a member is not the same as that of common mortals insofar as the scholar would never be able to abandon the critical spirit and its application to his own beliefs and convictions without ceasing to be a scholar. But his beliefs and his convictions thus influence more or less, directly or indirectly, consciously or unconsciously, the choice of his themes and subjects of research or the interpretation of his results in his approach. It is this impossible total indifference of the scholar in the social sciences, the fact that he is always personally involved in his research and the fact that he cannot totally abandon questions of value, which are thought to corrupt, as it were, the nature of the scientific truth to which he might have access.

This view is based on two ideas which seem to be equally disputable. The first is that truth in the natural sciences does not contain any subjectivity. The second leads us to believe that the social sciences have no way of realizing the coefficient of subjectivity resulting from the involvement of the researcher and that they are as a result incapable of producing knowledge likely to achieve agreement.

The first idea, which seems to exclude any subjectivity in the natural sciences, proceeds from the error which reduces his total disengagement because the scholar is obligated to neutrality in these sciences.

It often happens, however, that two specialists in the natural sciences, trained in the same school, working in the same laboratory and on the same research program can have momentarily different interpretations of the same results. This

difference in interpretation can be explained by the fact that one is more intelligent, perspicacious or simply more attentive than the other in the explanation of results. It may even be a matter of questions of intellectual honesty, moral rigor, even “monetary” interests. These are definitely so many subjective aspects which a person who considers scientific work, not in the way it is meant to be conducted, but as it is really and concretely conducted, cannot deny. Moreover, scientific activity involves the handling of concepts, theories, methods and sometimes material instruments. The skill of the researcher in this handling plays an important role in the value of results. Yet, skillfulness is also a subjective element in that it can vary and always varies from one researcher to another. It suffices to say then, in a more realistic and objective way, that in the natural sciences as well, the subjectivity of the researcher comes into play even if it does not do so in such an obvious or significant way as in the social sciences. This does not mean that the social sciences have no means of containing this share of subjectivity in the limits imposed by their claim to results likely to bring about agreement.

Now we come to the discussion of the argument on which those who see the involvement of the researcher as a valid reason for disputing the scientific nature of his results rely.

Indeed, researchers in social sciences are not totally disarmed faced with the consequences of the inclusion of subjectivity in their research. The arms at their disposal to do this are not necessarily the same as those of the natural sciences. Recourse to archival sources and documents, which can be written, oral, or audiovisual and which should especially be authenticated or authenticifiable, practices that can include field work (studies, questionnaire, focus group) in view of collecting reliable data, the cross-checking of these data, their interpretation and the discussion of this interpretation among peers, their critical analysis and acceptance, if necessary, of a repetition of the same process, the use, if needed, of procedures of quantification (formalization, statistical formulation, electronic processing) are so many ways which enable the social sciences today to provide the knowledge that they develop with a scientific value which is a prisoner neither of the arbitrariness of researcher, nor of caprice of his will, nor the vagaries of his competence, nor the relativity of his political, philosophical, religious, social and cultural convictions.

Besides, if society, and more specifically decision makers within society, trust and solicit more and more widely the social sciences, it can be explained in this way: Following the example of the natural sciences, they can provide knowledge allowing us to understand social phenomena; understand the share of necessity inherent in their emergence, their development and their disappearance; identify the objective tendencies which are expressed through this necessity; and, for all of these reasons, to act with a fairly acceptable effectiveness on man’s social environment; enable him to monitor and direct it as best as he can, in accordance

with his interests and aspirations, the evolutions which are produced there or which are likely to be produced.

A third aspect, from which some believe they can draw arguments to contest the legitimacy of the claim of the social sciences to be full-fledged sciences concerns the “objectivity” of knowledge in these sciences. By objectivity, we mean permanent interaction, the reciprocal influence of the subject and his knowledge. Knowledge that the subject in the social sciences has, always has an influence on him and on the conduct of his research, influence which, in turn, acts on knowledge.

For the supporters of this viewpoint, by making knowledge unpredictable in the social sciences and by reducing, as a result, the margin of foreseeability possible on the basis of knowledge acquired in these sciences, this reflection holds that the social sciences are less reliable than the natural sciences which, on the contrary, are thought to guarantee a strict certainty. But the uncertainty of knowledge and, therefore, the share of indetermination that we must carefully handle in all decisions based on knowledge, is not the monopoly, as it were, of the social sciences. We know, for example, that one of the main tendencies which have fundamentally disrupted the practice of physics and more particularly the concept of the scientific character is formed precisely by the appearance of indeterminist, probabilistic and relativist currents at the beginning of the 20th century.

Conclusion

The social sciences are sciences in their own right, but with a certain epistemological specificity. With respect to the question of whether they can legitimately claim to have the status of full-fledged sciences, we believe that we have begun to answer in reconsidering the concept of science itself and the criteria by which it is usually defined.

We have shown that the relevance of such criteria could only be accepted if they are relativized. The idea of a science capable of representing the reality of the world for us – with no additions or deletions – with complete objectivity is only a simple illusion. Once this is understood, the relative youth of the social sciences and the inevitably resulting consequences for their approaches and their methods stop being an unacceptable sin which would eternally condemn them to trial and error and uncertainty. In reality, this circumstance, which is a part of the history, but also the specificity of their subject, and of the role that the close relationship between the researcher and his field plays in these sciences, should be considered, but only to measure the not very important accomplishments by specialists of these disciplines in the development of the ways and means necessary to overcome these handicaps.

Currently, it is life itself which has decided the debate by the recognition which it has given to the utility of these sciences. Contemporary societies no longer need to be shown that the knowledge that the social sciences provide them on themselves

is useful and necessary, and sometimes even more so than in the natural sciences. Like these latter, the social sciences can make available to societies a knowledge which they can rely on to control their environment in an ever more intelligent way, find the perspectives of their evolution, and construct for themselves a future equal to the aspirations, expectations and priorities of men who live there.

Notes

1. We point out some useful works on epistemology to consult in the bibliography. But we would draw particular attention to the collective work written under the direction of Jean Piaget under the title *Logique et connaissance scientifique* [*Logic and Scientific Knowledge*] (Encyclopédie de la Pléiade, Paris, Gallimard, 1967). Aside from an important contribution by Piaget himself on the nature and methods of epistemology, we can read here enlightening epistemological reflections from the pen of specialists of particular scientific disciplines. We can also read with interest the work of Jeanne Parain-Vial (Parain-Vial 1985).
2. We are dealing with what we could consider as a “normal scientificity”, a concept that we have borrowed from François Russo (Russo 1983: 36-40).
3. On the comparison between political and scientific revolution in the work of Kuhn, see in particular pages 133-135, and on the concept of scientific revolution more particularly, see all of Chapter VIII entitled *Nature et nécessité des révolutions scientifiques* [*Nature and Necessity of Scientific Revolutions*]
4. On the subject of the critique of the inductive approach considered as pseudo-scientific, see: A. Virieux-Reymond (*op.cit.*, pp.38-40; Carl G. Hempel, *Éléments d'épistémologie* [*Elements of Epistemology*], Armand Colin, 1972, pp.15-19; W.M.O'Neil, *Faits et Théories* [*Facts and Theories*], Armand Colin, 1972, pp.172-173, 278-279; Robert Blanché, *La logique et son histoire* [*Logic and Its History*], Colin, 1970, pp.78-79. Karl Popper, *Logique de la découverte scientifique* [*System of Scientific Discovery*], Payot, Paris, 1973.
5. See on this subject, Claude Bernard, *Introduction à l'étude de la médecine expérimentale* [*Introduction to the Study of Experimental Medicine*], Paris, Garnier Flammarion, 1966; see also G. Canguilhem, *La connaissance de la vie* [*Knowledge of Life*], Paris, Vrin, 1965, pp.19-21.
6. It would be interesting to read pages 157 to 166 which are devoted to this concept.
7. The Circle of Vienne is a philosophical trend which started in Vienna, Austria at the beginning of the 1930s, with thinkers like R. Carnap, O. Neurath, M. Sclick, on the progress of the elimination of metaphysics. For more complete information on this school of thought, read, among others, P. Jacob, *L'empirisme logique* [*Logical Empiricism*], Paris, Éditions de Minuit 1980 ; Jean F. Malherbe, *La philosophie de K. Popper et le positivisme logique* [*The Philosophy of K. Popper and Logical Positivism*], Paris, PUF, 1979, D. Lecourt, *L'ordre et les jeux: le positivisme logique en question* [*Order and Games : Logical Positivism in Question*], Paris, Éditions Fasquelles, 1982.
8. On the ideological and political unthoughts of scientific work, read, among others, the collective work of Hilary Rose, Steven Rose, Jean- Marc Levy-Leblond et alii, published under the title *L'idéologie de/ dans la science* [*Ideology of/ in Science*], Paris, Seuil, 1977.

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4

Reasons and Causes: Wittgenstein versus The Myth of Causal Explanation in the Social Sciences

Pierre Bouda

According to the normal way of looking at things, science is an activity with the objective of bringing to light the causes of phenomena. In this sense, a scientific law is a formula which establishes a causal link between phenomena of type A and phenomena of type B, the former being considered as the cause for the latter. Scientific research protocols are, therefore, procedures thanks to which we first discover the causes of the determined phenomena, and we then verify that they are the real causes of the phenomena considered. That would explain the relevance and effectiveness of science. We cannot then be surprised if the social sciences are sometimes tempted by the desire to develop on their turf an exact reply to the causal explanation in force in the natural sciences. To what extent is that justified? Is the concept of cause, more or less relevant to render an account of what happens in nature, a judicious point of view to observe and understand human action? Wittgenstein, who before devoting himself to philosophy first practised science, considers that the idea of causality leads to an erroneous perspective in the attempt to understand human fact.

What is Cause?

The idea of cause is originally a legal idea in the exact sense that the search for the cause is, originally the process which leads to assigning responsibilities. Designating the cause means denouncing the guilty party, the author of an act. We, therefore, understand that the primary meaning of the idea of cause is the strong or metaphysical meaning. Literally, we can say, the phenomenon which is the cause of another is the

one which is “responsible” for it insofar as it produced it. The effect is the work of the cause which is thus recognized as having a creative power, a certain virtue. The analyzed fact is explained from the moment when one is able to indicate by particular procedures that it comes from another such fact which is its substantial origin. We understand how the concept of cause thus understood was considered to be metaphysical. In the spirit of positivism, the idea that a fact produces another is based on the belief in occult powers which operate in phenomena, since the causation thus understood is an extrapolation based on the experience of the regular succession of two series of events. It is then clear that if experience shows us the succession, it does not show us causation if this implies something other than a constant relationship of precedence. In accordance with phenomenism¹ which defines it, positivism denies all epistemological relevance to the notion of the cause understood in the metaphysical sense, and proposes another concept which brings causality back to the idea of regular succession. A phenomenon A is the cause – in this weak sense or positivity – of a phenomenon B if a substantial series of observations shows that A is regularly followed by B. The norm here is experience thanks to which the affirmation of the existence of a causal relationship can be monitored and authenticated. We are thus dealing with an idea which is really operative insofar as it enables the construction of sufficiently precise criteria of validation of scientific statements. This is the *de facto* importance of induction as an effective procedure for the establishment of scientific propositions which confers on the positivist concept of cause its epistemological dignity. Inversely, the limits of induction from the view point of logical analysis affect the philosophical value of this weakened causality which comes, as a result, in the form of a fully assumed modesty. Wittgenstein, who has a tendency to discount the empirical and the factual in favor of the transcendental and the formal, the contingent in favor of the inexorable, and thus the natural sciences in favor of logic and mathematics, always refers to the positivist concept of cause when he speaks of causal explanation.² And he considers this mode of approach to reality as more or less relevant to the study of the phenomena of nature, where it has produced an attitude and results in accordance with the spirit of the time and his civilization which he judged to be in decline. But he persists also in thinking that the generalization of the causal explanation to the study of the human fact is fundamentally excessive, and constitutes a permanent source of errors and blatant mistakes that he spots in Freudian psychoanalysis and in anthropology *à la* Frazer.

Wittgenstein’s General Attitude towards Science

Wittgenstein’s intellectual adventure began with engineering studies, and particularly by a marked interest in aeronautical issues. Then his curiosity turned towards mathematics which made up this “foundation” of physics. From there, he was

attracted by discussions on the foundations and logic of mathematics. And he ended up, in a more or less predictable way, with philosophy. This allows us to understand in a way Wittgenstein's attitude towards science: an almost natural critical detachment. Nothing was farther from his mind than this bewitchment that some philosophers undergo with respect to science.³ Since he has practiced science, he is less susceptible than others to being bewitched when he reflects on its spirit and value.

And contrary to the most widespread opinion, he does not see in science, its spirit, its method and its results, the quintessential substance of reason, the most complete product of a reason which has reached maturity and illuminating reality for man, driving away obscurity, ignorance and error, and cheerfully bearing the effort of man marching towards happiness.⁴ In his mind, science unmistakably contains something which is headed in the direction of the good. But, on the one hand, this aspect is unfortunately fairly misunderstood; and on the other hand, it is suicidal to see nothing but this. This is the way that he writes about the scientific method:

Science: enrichment and impoverishment. A unique method sets aside all the others. Compared to them, they give the impression of being indigent, of making up at most preliminary stages. You have to go back down to the sources, to see them all next to each other, those which were neglected and those which were preferred (Wittgenstein 1984:74).

In other words, the hypothetical-deductive explanation has an obvious heuristic power, and its use was indisputably fruitful in that it has produced a considerable amount of precise knowledge on facts; but its quasi-exclusive culture is, in a completely obvious way, an intellectual impoverishment, insofar as other spiritual sources of understanding of the worlds were unfortunately sapped. The task of philosophy is precisely to maintain, in a way, the memory of these forgotten methods. Indeed, for Wittgenstein, philosophy cannot have the objective of the justification of possibilities that were realized; what it should do is to open the largest space possible of possibilities within which it shows what has come true as a simple particular case which has only one completely relative privilege which is essentially contingent on the other possibilities. With respect to the spirit of modern science, what he wrote in the *Tractatus* can be considered to be his most consistent opinion on the subject:

At the basis of all vision of the world of the moderns, there is the illusion that the laws of nature, as they are called, are the explanation for natural phenomena. Thus, the laws of nature remain as something intangible, just as the Ancients did with God and Destiny. And both are, in truth, right and wrong. The Ancients are, in any event, clearer insofar as they recognize a

clear stopping point whereas with the new system we should have the impression that all is explained (Wittgenstein 1993:63-72).

What science made the mind of the moderns get used to was considering that the transparent world can, *de jure*, be explained. In other words, our relationship with things and beings is one of triumphant arrogance. We have the tendency to see in the laws that science formulates absolute knowledge of phenomena. And we think that nothing can withstand the methods of investigation that we have created to study and understand the world. Yet, Wittgenstein, in his stubbornness, feels that this attitude is far from being wise. In any event, it is not the only possible one. And in his mind, the Ancients were much wiser insofar as their attitude translated a clear awareness of the limits of knowledge. As a result, Wittgenstein stressed the fact that if today we believe that if we have the positive, effective, and absolute explanation of the world in scientific laws, this is not so much because of the proven relevance of our scientific theories but because of a new attitude vis-à-vis reality, a new point of view on knowledge, on man, etc; a new form of life. What has changed then is not the extent to which science has become effective, it is our relationship with the world. We should thus not think, when Wittgenstein contests the relevance of the explanatory model in the social sciences, that he is establishing implicitly or explicitly a hierarchy in favor of those who study nature; we should not think that he considers anthropology as a discipline which outlines knowledge of less value or less interest than physics, for example.

The Denunciation of Physicalism

We should clarify certain aspects of Wittgenstein's attitude with respect to causal explanation. Concerning the concept of cause as the expression of a simple relationship of regular coincidence between two phenomena, Wittgenstein stresses the Humian⁵ character of the procedure which leads to the assignment of a cause. From the moment when it is only the repeated observation which validates a proposition of this type, the causal explanation cannot have the type of necessity which we generally grant it. This illusion explains the prestige which the classical model of scientific explanation enjoys, such as it is applied in a paradigmatic way in physics. In a certain sense, we can say that what motivates Wittgenstein's in-depth intervention in the social sciences is the constant desire to denounce the characteristic obsession of our time, that is to import everywhere the procedures of validation used in mathematical physics – physicalism. Schematically, the model in question here consists of observing a certain phenomenon which poses a particular problem (the objective of the observation being to define and clarify the problem), to generate a hypothesis which clears up the difficulty by indicating the cause of the phenomenon; and finally testing the hypothesis by conclusive experiments. Thus, the explanatory hypothesis is the supposition of an unapparent

mechanism which renders an account of what is apparent; we assume a phenomenon which was not perceived (and perhaps which cannot be observed directly), but the effects of which correspond to what is observed; we assert a fact of which the phenomenon is the effect; a cause is assigned to the observed phenomenon. And experience is a test of the hypothesis which is thus confronted with the facts, and is validated only to the extent that the facts do not formally invalidate it. In these conditions, Wittgenstein considers, as we have already noted, that the attitude which consists in adhering in an unconditional way to scientific theories, and considering that all the other positions on nature are nothing but the expression of ignorance is unfounded. Particularly, the uncontrolled wonder vis-à-vis scientific discoveries, and the belief that “it should necessarily be so,” all of that is nothing but mythology. Physicalism is thus an ideological tendency which is eminently misleading.

The Myth of Causal Explanation in the Human Sciences

Wittgenstein does not dispute the idea that causal explanation is an extremely powerful practice which leads to considerable knowledge of phenomena. He does not deny the merits of a procedure which has proven itself in the natural sciences and, oddly, in physics. But what he does say is that the idea that we have of it is largely mythological. With respect to this idea, there are two observations which are particularly significant to Wittgenstein’s mind. The first is that we take the causal explanation for what it is not: as the unique source of knowledge of any object. According to the author of *Philosophical Research*, the causal explanation is only one sort of relationship with the world among so many others. The second observation is that we consider fairly complacently scientific knowledge thus accumulated as intangible truths, as the only valid knowledge of the world, the other forms of knowledge thus being necessarily permanently rejected as errors without any importance. Wittgenstein attributes the root of these two attitudes to the seduction that absolute, encompassing and definitive explanations work on the human mind. This is the way we are: our mind is uncontrollably attracted to systems; all knowledge that is presented as reductive syntheses says: “all comes back to this”.⁶ Wittgenstein first points out this detrimental fascination for causal explanation in psychoanalysis, and he diagnoses confusion in Freud’s work between reasons and causes. Freud was constantly concerned with presenting psychoanalysis as a science in the classical sense of the term. For example, he wrote in *Resistance to Psychoanalysis* that psychoanalysis “is based on the patient and laborious observation of facts pertaining to the world of our perceptions” (Clément, Bruno and Sève 1977:24). He states and repeats that psychoanalysis is a scientific psychology (and not philosophical or speculative) which uses the canonical procedure of science. He claims to update the cause of human action in a completely experimental way:

- a) He observes the individual, particularly an individual suffering from any neurosis;
- b) He puts forward the hypothesis that a given unconscious desire the “action” of which explains the pathological behavior of the patient;
- c) He verifies this hypothesis by efforts, which he admits are sometimes substantial, to obtain the agreement of the patient.

In the 1930-1933 lectures, Wittgenstein asserts that by attitudes of this sort, Freud is creating confusion between the reasons and the causes, at the root of a “terrible waste” (Clement, Bruno, Sève 1977:316). He understands the difference which exists in his mind between a cause and a reason in *The Blue Notebook*: “The suggestion according to which your action has such or such a cause is a hypothesis. The hypothesis is well founded if one has a certain number of experiences which, on the whole, are in agreement to show that your action is a regular series of certain conditions that we then call the causes of the action. To understand the reason that you had for formulating a certain statement, to act, etc., no number of corroborating experiences is necessary, and the statement of your reason is not a hypothesis” (Clément, Bruno and Sève 1977:15). In other words, the cause is a hypothesis which means:

- a) it is never known immediately, but always in an inductive fashion, after a number of substantial corroborating experiences;
- b) that it is never certain since it is derived not from a logical procedure put from an empirical process.

On the contrary, the reason for an action can be known immediately, and with certainty. As a result, if Freud discovered the causes for the behavior of individuals, and not reasons, he could not consider the acceptance of a subject as proof of the fact that the explanation which he proposed for the problem is accurate. If it is in the nature of a cause not to be known by the subject, we can obviously not take as proof of the accuracy of an alleged cause the fact that the individual recognizes himself that such was not the cause of his behavior. On the other hand, if the explanation of the action of the subject can be recognized as accurate by him, and if this recognition can be considered to effectively prove the explanation, this means that it is not an explanation by the cause but by the reason which can be known with certainty by the individual. The result of this confusion between reason and cause is a sleight of hand that Freud allows himself: when the subject agrees with the explanation given by the psychoanalyst, this acceptance is considered to be a confirmation (exactly as if it were a matter of a reason that the agent intuitively knows); but when the subject disagrees with the psychoanalyst's suggestion, this disagreement, instead of appearing as an invalidation, passes on the contrary for being of no importance, and even normal, since a cause cannot

be recognized. Freud analyses reasons as causes which can be the object of a hypothesis (as in the experimental method) which is tested on the subject, and which is confirmed if he or she accepts it as the real explanation of his or her problem. Unconscious psychic entities are characterized by the fact that they are both unknown and can remain unknown by the subject (in the same way as the cause), and recognized immediately by the individual who convinces himself that this was indeed the reason for which he acted in a particular way. For Wittgenstein, this constitutes a deception because the causes and reasons are not discovered by the same procedures.

Another time, Wittgenstein has the chance to denounce the unfortunate tendency to transfer the model of causal explanation from the natural sciences into the human sciences. An analysis of myth by Frazer provides him with an occasion to denounce this tendency. What he contests is the idea, implicit in the theory of causality, that the action has one cause and necessarily one cause. For him, men are beings capable of acting for various reasons. Identical human behaviors are not necessarily related to identical causes. It is this idea, false to his mind, which supports the idea of the possibility of a general theory of the human fact as Frazer thought he was able to provide with respect to myths.

Frazer analyzes myth in terms of erroneous knowledge. Myth, according to him, is the science of primitive peoples. Man, faced with natural phenomena, is necessarily overcome by a desire to know, a curiosity which is satisfied among advanced people by science and philosophy and among primitive people by myths. These are, thus, erroneous explanations of a phenomenon, whether they be about human life or those of external nature. Ignorance and incomprehension in which men find themselves vis-à-vis events and facts explains that the way in which they understand the world initially is incorrect. On the other hand, Frazer considers ritual practices as a means of indirectly reaching mental states which carry them, and because of this he believes that identical attitudes are related to identical mentalities, that identical customs can be explained by identical psychological motives. Besides the fact that this theory of the myth clashes head on with Wittgenstein's antipsychologism, it is also disputable on three points in the view of the author of the *Tractatus*. In the 1930-1933 lectures, he considers that:

- Frazer is mistaken when he posits one and only one "reason" in the sense of "motive" which leads men to complete a particular action;
- Frazer is committing an error when he affirms that "the motive is always to obtain something useful";
- It was a mistake to suppose that the reason for which, for example, the tale of Beltane's fete "made such a great impression on us" is that it "evolved out of a party during which a real man was burned" (Moore 1997:129- 130).

We can thus see from this critique that Wittgenstein reproaches Frazer for approaching the human fact with the way in which the physicist studies phenomena in mind. According to him, it is only the confusion of reasons and causes that explains why Frazer believes in the existence of a unique and general explanation for a kind of customs. Only the false analogy between the analysis of human action and that of phenomena of nature allows us to understand what Frazer is doing when he interprets myth in a one-dimensional way in terms of knowledge and error; when he thinks that for customs each time there is an explanation which is the only explanation, when he considers that there are customs which should allow a single explanation. On the other hand, Wittgenstein considers that the use of causal thought sometimes surpasses the weakened meaning to assume the strong meaning. He denounces, for example, the causal theory of the sign which he sometimes terms as magic: it is the idea that the sign (i.e. the symbol in the broad sense, for example, a rule) "would act as a drug" to push men to action. The symbol, the rule would necessarily cause the action, the practice in such a way that the theory of causality would be perfectly sufficient to render an account of a human fact. Yet, Wittgenstein maintains that the rule is always the subject of an interpretation linked to use. Like direction signs, the rule only means something because there is a constant use which consists in giving it such or such meaning. Can we really say that the path leads somewhere even if no one goes there? Does a rule do such and such a thing even if no one follows it? At the beginning then, there is a use, a "form of life." And we do not have to try to explain hypothetically, but to understand, i.e. describe. The meaning of a practice is there, in the practice itself. What we seek to understand is there, under our noses: we have only to adopt a certain perspective, to arrange in a clarifying order the elements which have always been there.

Conclusion

According to Leibniz, reasons, unlike causes, influence without necessity. For Wittgenstein, necessity is already lacking at the level of the cause. The idea that the causal explanation contains an effective necessity is already a myth for him. In the analysis of reasons, it is *a fortiori* incorrect to expect an absolute necessity. The description that Wittgenstein considers as a more relevant method than the causal explanation of human facts is meant to take into account the essential presence of meaning in the action of man. In Wittgenstein's thought, a practice is a meaning, and this is a function of a form of life in which it is integrated. That is why the interpretation to which it should be submitted is not a subjective comprehension *à la* Dithley, but an objective comprehension which is the ability, as Jacques Bouveresse says, to participate in a form of life. To describe a human fact, to understand it in Wittgenstein's meaning, is then to construct an enlightening configuration of elements of the form of life in which this fact acquires a given meaning.

Notes

1. The phenomenon is, etymologically (the Greek *phainomenon* designates what appears to the senses), the appearance. Thus, phenomenalism is the conception of science as an undertaking to describe appearances. And, according to positivism, the objective of science is to provide schemas, models which effectively give an account that what we perceive, in being careful to avoid all conjecture on powers, hidden entities which might be the origin of facts. In the domain of the social sciences, we sometimes see a conception of positivism which defines it as an attitude or thought which consists in considering that the social sciences should adopt at least the spirit of the natural sciences, if not the methods. This assimilation of positivism to methodological monism can obviously be explained by the historical fact that the name of Auguste Comte is associated both with positivism in the social sciences and methodological monism. It is, however, clear that there is not a necessary link between phenomenalism and methodological monism.
2. He writes that “causation is (–) what we observe by experiences, by observing the regular coincidence of process” (Wittgenstein 2004:196).
3. “Scientific questions can be of interest to me, but never really captivate me. Only conceptual and esthetic questions can have this effect on me. I am basically indifferent to the solution to scientific problems; but not to problems of the latter sort” (Wittgenstein 1984:94).
4. In *Culture and Value*, he writes: “It is not devoid of meaning, for example, to believe that the scientific epoch is technical and the beginning of the end of humanity; that the idea of great progress is self-delusion, as is as well the idea of complete knowledge of the truth, that in scientific knowledge there is nothing good or desirable, that humanity which aspires to this knowledge is falling into a trap. It is not absolutely clear that that is not the case” (Wittgenstein 1980:56).
5. It was David Hume who brought up what is called the problem of induction. He asserted that there is no logical relationship between factual observations already made and an observation to be made. Thus, induction has a psychological, and not a logical basis. It is the habit of seeing things happen in a certain way which leads us to think that they should always happen in the same way. Therefore, it is only the habit of seeing the sun rise each morning that makes us say that it will rise tomorrow morning. From this perspective, nothing allows us to claim that, narrowly speaking.
6. Wittgenstein has a developed sensitivity to understand the differences, to, as Kraus says of himself, separate and distinguish. Characteristically, he confided in Drury with the following: “Hegel always seems to mean to say that things which seem different are in reality the same, whereas what I am interested in is showing that things which seem to be the same are in reality different.” (Bouveresse 1991:10) He considers that the philosopher’s task is to resist the terrible tendency to theorize.

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5

Scientific Logics and Methodologies

Gbocho Akissi

Logic: From Refusal to Evocation

Unfavorable prejudices with respect to logic have kept it at arms-length from methodological problems for some time. During the Renaissance, Montaigne accused Aristotelian syllogistic logic of making minds “muddled and smoky.” German idealism rejected it. Kant and Hegel, for example, finding it closed, complete, sterile and not fruitful, substituted, respectively, a transcendental logic and a contradictory logic. Researchers in social sciences have followed closely behind them, suspecting logic of evading the rich empirical reality and then undertaking to construct methods particular to their sciences. All this primitive, age-old mentality even led thinkers to relativize logic culturally and ideologically. This scorn resulted in both ignorance and a belated evolution of logic.

Of late, the boundaries of this ostracism are more and more retracting and the idea of a contribution of logic to methodological and doctrinal research no longer negatively clashes with the consciousness of the researcher. This change is due to three principle factors: research in logic by Bertrand Russell, Gottlob Frege and Ludwig Wittgenstein has shown that this discipline cannot be reduced to the syllogistic, which is only one part of it. Next, the success of methodologies in physical sciences, resulting in part from the contribution of logic and mathematics to quantification, led to lessening the discredit against logic. Finally, Quine’s reflections on the philosophy of logic showed its involvement in all undertakings in radical translation. Radical translation being the decoding of statements of a subject language into those of a metalanguage different from the source language, all research in the field can be considered as an effort at radical translation. Quine argued why and how logic – especially bivalent logic – is embedded in such a venture without, however, being relative and vivid.

This chapter will attempt to initiate thought between logic and methodology. It comprises the key concepts to which the concept of logic refers, i.e. “reasoning,”

“argument,” and “proof.” By defining logic as the science of reasoning, I determine the nature of reasoning and identify the sophistic pitfalls which are strewn about the field of argument. A series of rules designed for researchers and those who write scientific texts are proposed.

Logic and Functions of Language

Like all terms in ordinary language, “logic” is an ambiguous concept. Out of the various possibilities, we will choose five essential meanings.

In its scientific sense, which is the subject of this work, “logic” means “the operation of the mind” and includes three fundamental concepts: calculation, rule, combinatorics that we will examine through the concepts of “reasoning,” “argument,” and “proof.”

In the common meaning, i.e. *doxa*, “logic” refers to “opinions,” “personal vision,” or “group vision.” More basically, the term refers to what I would readily call “ontological assumptions” of a linguistic community, an ideological group, a culture, a people, a civilization, a theory, i.e., the set of beliefs, values and hypotheses which serve as their cognitive or pragmatic horizon, premises or postulates of arguments. The assumptions may be relative, as, for example, in the expressions, “I have my logic, you have yours;” “European logic,” “African logic,” “Asian logic;” or in “logic of political parties.”¹ They can also be absolute, objective, made of the set of universal values and beliefs of humanity; for example, “logic of human and citizen’s rights.”

In a third meaning, “logic” is a synonym of “method,” or “approach,” “process of scientific activity;” or “conditions of possibility” as in the expression: “Logic of scientific discovery.” In another sense, it refers to the quiddity or essential meaning of a concept. In the expressions such as “logic of domination,” “logic of politics,” “logic of forgiveness,” for example, the term is used to refer to both their quiddity and the conceptual constraints thereby linked as the very result of their meanings. Finally, “logic” designates the structural or structuring organization of something; e.g. “logic of cities.” There are certainly semantic affinities between the last meanings of the concept, just as there are other meanings. But it is important to draw a line of demarcation between the second which is subjective and the first which is objective.

The physical and social sciences are all aimed at knowledge, thanks to which we can hope to leave behind ignorance, a source of irritation and intellectual or pragmatic confusion. Knowledge, however, can only fulfill this function provided that it is the expression of the truth. The relationship between the informed subject and the subject of acquaintance or knowledge, the truth cannot be sought in a particular science, each having its particular subject according to which it determines its criteria of truth. Despite their diversity of empirical content, these sciences refer no less to the requirement of non-contradiction and consistency. These values of scientific rigor vest the various regions of the episteme, and consequently appeal to logic, the science of valid inferences, the sources of conditions of consistency and non-contradiction.

As a being in need of knowledge and action, man is separated, spatially and temporally, from others for whom, however, he would like to have a relationship

with. The physical confrontation in the form of violence (war, conflict or any other form of duel) represents a form of relationship, but one which is self-destructive precisely because violence is a plan to eliminate others. The true relationship that men are searching for is thus not provided by physical violence but by language. It is the mediator which is both necessary and sufficient by which a bridge is established linking one mind which is speaking to another mind which is listening, understands, and responds. Ludwig Wittgenstein, in his *Investigations philosophiques* [*Philosophical Investigations*] refuting the thesis of a private language, showed its social character, defining man as an essentially linguistic being. Indeed, there is no realm of his activities, whether they be serious or not, which does not proceed from and return to language since he uses it to interpret his relations with others, either in the form of questions to answer, problems to resolve, orders to give, or information to communicate. Using the analogy of the toolbox, Wittgenstein translates this diversity of functions, or “games of language” that can be reduced to four essentials:

1. Directive function: language, in a propositional, directive or argumentative form is used to give orders. “I order you to leave” (propositional form); “Go away!” (canonical form of the directive order); “I ask you to leave because I want to close the door” (argumentative form). In this function, the language first targets not the truth of the assertive propositions, but rather obedience (or lack thereof), execution (or lack thereof) of an action by the person to whom the order has been suggested. We do not characterize an order as true or false. We carry it out or refuse to carry it out.
2. Expressive function: here, the play of language is a form of life by which one expresses feelings. “I like him; I like him because it is he.” The epistemic values of linguistic entities of this function are the need for comfort, sympathy, sharing and not of truth, etc. You do not immediately respond, “it’s true” or “it’s false” to someone who tells you, “I’m hungry” or “I’m sad” unless you are very cynical!
3. Performative function: the speaker accomplishes an activity by the fact of saying: verbs such as promise, “solicit,” etc. accomplish the act of promising, of soliciting; etc. Thus, the justice of the peace, speaking to a man and a woman in front of him on the occasion of given ritual circumstances, “I pronounce you man and wife,” establishes the relationship of a couple by the performative word.
4. Communicative function of information. This function is accomplished by formulating, either affirmatively or negatively, proposals or structured sets of proposals as, for example: “A torrential rain pounded the city all last night or The flight to Cotonou was delayed because a torrential rain pounded the city all last night.” This function is concerned with entities of language (propositions, statements, endowed with the value of truth or arguments composed of propositions or statements). Science being the field of the episteme of this function, the subject of the study of logic is science.

The Pragmatic Function of Language

The presentation of the diversity of uses of language in four functions would lead us to believe that they are applied mechanically, that each region of the episteme corresponds to a sole and unique function. This is not the case. Certainly, the division of uses of language into four functions elucidates for us what is done or can be done with language. But such a division, though indeed pedagogical, is not illustrated in daily life where any statement or discourse can exemplify if not all, at least two or three of these functions. For example, expressive language includes an informative and directive dimension. The statement, I'm hungry, would not provoke sympathy (or lack thereof) if it did not have informative content. Moreover, it addresses the listener in this form: "give me something to eat." In a similar fashion, informative language promotes a directive thrust. The statement, "Classes at the university have not started up again" leads to a plurality of behaviors or actions in various listeners: work on other activities, procrastination, etc. Placed in its canonical form, the directive inferred is: Do something else. Even performative language, in addition to being informative, (the priest or the mayor informs the world of a new matrimonial relationship by witnessing the event) includes the directive function, inviting the couple and the audience to a certain type of conduct.

An exhaustive examination of the relationships between the language functions will result in the following situation: communicative and directive functions are embedded in all the others. The former deals with knowledge, science and the latter with action. They both place us in front of the traditional couple of the human cognitive experience: science/action, truth/action, know/do are included in this periphrase: communicate to have it made or done.

To communicate is to share. The word, which appeared in the work of Nicole Oresme around 1370, referred to the pooling of currency – objective data. In linguistics, pooled objective data are information that is shared by interlocutors in a relationship of dialogue with the effect of modifying their common cognitive environment; the information transmitted is aimed not only at reducing their degree of uncertainty or ignorance, but also at creating a human symphony. It is not rare to find in this dialogue-based relationship of ideas an affective relationship where feelings, a common presence, a warm feeling of togetherness – values which as may generate action as much as truth – are shared. What is the purpose of communicational truth and passion if not action? Truths only have meaning and relevance if they are used, dealt with to clarify and inflect action. Of what use is a feeling if it is not used towards a cause, an action? At the end of the day, the purpose, the vocation of language lies in the pragmatic function from which the other functions are detours.

In the final analysis, a linguistic communication, an invitation to action, is addressed to either man's cognitive region (epistemic beliefs, reason) or his conative region (desires, emotions, passions, pathos) or to both, in hopes of causing the action or the behavior sought, whence the functional entanglement above. The link between the informative and the directive will only lead to action if it convinces or persuades. Will the

problem of conviction or persuasion concern the propositional form or the argumentative form of the informative function? Can a series of statements, even repeated, extract lasting support? Should I say, “I want peace, I want peace, I want peace” to convince or should I provide arguments?

To convince, says Jean-Blaise Grize, is to lead someone to recognize the truth or the accuracy of a fact or its necessity; whereas to persuade is to lead him to believe, to do, to want something (Grize 1996:8). How can we lead this person to X other than by proof, demonstration, relevant reasons? It is thus not by the proposition, but by the argument that the conviction or the persuasion is established. Argumentation, says Grize, “is a chain of arguments, i.e. the presentation and articulation of facts in favor of a given thesis or against it” (Grize 1996:8). The science which defines the conditions of presentation and articulation of these facts is called “logic.”

Logic and Reasoning

In reading this title, “logic and reasoning,” the reader will probably have the impression of a pleonasm in that logic is defined as the science of reasoning. The impression is not at all justified. Reasoning is a special type of thought where an inference is involved, where conclusions are drawn from premises, postulates or axioms. The logician is concerned with the accuracy, or lack thereof, of reasoning by asking himself questions like: does the conclusion follow from the premises? The argument is said to be valid in the case of an affirmative response, but invalid or sophistic in the case of negative response. Thus, one can conduct reasoning that is not logical, just as we can formulate logical expressions that are not necessarily reasoning. By way of an illustration of the first case, let’s consider the following arguments:

1. Deductive Reasoning (inference or conclusion of *one* from *all*)
 - a) All men are mortal (premise or postulate).
 - b) Socrates is a man (premise or postulate).
 - c) Thus, Socrates is mortal (conclusion or inference).
2. Deductive Reasoning
 - a) Everything that is rare is expensive.
 - b) An inexpensive horse is rare.
 - c) Thus, an inexpensive horse is expensive.
3. Inductive Reasoning (inference of *all* from *one* or *several*)

All crows are black because we observe some that are.
4. Deductive Reasoning
 - a) All believers are generous
 - b) Jean Le Croix is a believer
 - c) Thus, $2+2=4$

The first argument – logical, i.e., valid in that the conclusion necessarily follows from the premises – confirms our belief in our rationality on behalf of which we emit doubts as to the accuracy of the second and the third, and we laugh at the last because it is so cockamamie. Why do we accept the first and reject the others? It is the task of logic to answer this question. What is reasoning or argument? Before we determine its nature, we should stress its importance in our human activities.

Can we get along without arguing, without reasoning? How many times have I heard students in Côte d'Ivoire saying: "We don't want to reason; we want to eat;" "Where is reasoning going to get us?," "What use is reasoning?" Such questions seek to avoid the response that they all fear: to know or to understand. Isn't the objective of the study of logic and of all educational disciplines to help us to know or understand, to satisfy the need of curiosity which is characteristic of human existence? But what good is it to know when there is neither a model nor an ethic? What good is it to understand when the understanding leads to discouragement or skepticism, or shirking responsibilities?

This rushing to judgment, expression of a fact, of the unhappy, jaded Ivorian consciousness lead, however, to a paradox similar to that of the negation of philosophizing. To those who said that philosophizing was not necessary, Aristotle gave this argument: "If we should not philosophize, then we must philosophize (to show that we should not philosophize). Thus, we should philosophize." In a similar fashion, if we should not reason, then we should reason (to show that we should not reason); thus we must reason. Not wanting to reason, unless we resort to force, is still reasoning since we give reasons for or against, whether these reasons are relevant or not. To not want to reason but rather to eat makes no sense since we should argue, i.e., give reasons to support the idea that we should eat rather than reason. And thus to proceed as such is reasoning. Whether we are philosophy students or not, we are condemned to reason, because reasoning or argumentation is part of our-being-in-the-world. It is a fundamental activity of our life in relation to our fellow man. Human life is full of significant decisions and choices, with respect to what there is cause to do, or have done or believe. To decide if we should send our girls to school or not, if we should vote for or against the death penalty, if we should believe or not what an official says, if we should carry out a task or not, etc., all this requires that we provide arguments, i.e., reasons. To establish or refute a fact, a statement, either to convince an audience or to dissuade them from doing something or to inform them of something is part of our daily activities. We carry out these activities by giving reasons; and to give reasons is to suggest arguments or reasoning.

From this perspective, our scientific theories of the physical or human world and our daily linguistic activities represent sets of arguments, or reasoning.

An argument, in the sense that we will use it, is a mental operation by which intelligence makes an inference meant to be logical in view of establishing that something is or is not the case. By "inference," I mean that a conclusion (what we are trying to establish) is derived from one or more premises. The inference is logical if and only if there exists a link of organic necessity between premises and conclusions.

Suppose that you wanted to establish the following: “Mr. Tartempion cannot vote.” You should only do it by giving reasons in view of answering the question which may be asked of you, “Why?” Your reasons can be, for example:

- (1) Only those who are registered on a voting list can vote.
 - (2) Mr. Tartempion is not registered on a voting list.
- (Therefore, Mr. Tartempion cannot vote).

Such an argument is a unit of reasoning in that it includes one and only one inference, or one and only one conclusion, namely: “Mr. Tartempion cannot vote.” It is formulated in a hypothetical-deductive or syllogistic form. There are other forms of presentation that operators or fonctors of premises and conclusions provide. Operators of arguments introduce the premises of an argument: “because,” “since,” “for,” “for the reason that,” “insofar as,” or equivalent expressions. Among the expressions introducing the conclusion of argument, we find: “therefore,” “consequently,” “thus,” “it follows that,” “it ensues that,” “we can conclude that,” “the result is that,” or their equivalents.

There are, however, arguments in which there is no expression indicative of a conclusion or premises. Here is an example from Fichte:

Practical reason is the root of all reason. The laws that govern the activities of reasonable beings are of an immediate certainty; their world is only certain because these laws are certain. We cannot renounce these links without the world and ourselves being plunged into absolute nothingness; it is, in part through our morality that we come out of this nothingness and that we maintain ourselves above this nothingness.

The first sentence of this argument is the conclusion. How do we know this? By paying attention to the context, by reflecting on the meaning of the words and, more generally, in asking ourselves these two questions: (1) What exactly is the point that the speaker wants to establish? (2) What reasons does he give? A practical way of identification is to insert (verbally or mentally) between two phrases or segments of phrases expressions such as: “The reason for this is that,” “for,” “because.” These are only a few suggestions. No formula; philosophical thought or logical analysis is not a mechanical activity with applications of fixed criteria.

There are three and only three ways of presenting an argument: (1) either the premises are first stated followed by the conclusion; in this case the conclusion is generally identified by its fonctor; (2) or we may first state the conclusion and end by the premise or the premises preceded by their operator; (3) or we may place the conclusion between two premises. These considerations show that there are no rules concerning the order of precedence of conclusions and premises. The order depends on the intention of the speaker, what he considers to be the most effective to achieve his objectives. If, for example, the speaker considers his conclusion sufficiently reasonable for his audience, he can first state it and then provide the reasons for it. But if he plans to refute an idea or an opinion which the audience holds, it will be more judicious to begin by stating his premises

or reasons, from which he will deduce his conclusion. He will then have a much greater chance of getting his audience to accept a different conclusion from the one which it previously supported.

Finally, we must distinguish between the logical connectors such as the conjunction, the exclusive or inclusive disjunction, the conditional, etc., which are operators for the formation of compound clauses (in formal or informal logic) and the operators or *foncteurs* of arguments. The conjunctive clause: "Winks and works of art make up a language" does not represent an argument because it includes no inference, i.e., the clauses:

- (1) If art is expressive, it makes up a language ; and
- (2) Because art is expressive, it makes up a language.

The clause (2) represents an argument in the eyes of the *foncteur* "because" which provides a reason. This is not the case in (1) which expresses a conditional hypothesis.

Logical operators are not indicative of arguments, but form complex clauses likely to enable their construction.

- (1) "Winks and works of art are languages."
Winks are language.
Thus, works of art are language.

In a more complex way: "Since winks are languages, works of art are as well, given that the former and the latter are language."

- (2) Only those who are registered can vote.
Mr. Tartempion can vote.
Therefore, Mr. Tartempion is registered.

The arguments, no matter what form they are stated in, are formulated using two methods: the inductive method and the deductive method. The former is a generalizing inference of conclusion from specific premises. Induction concludes the specific from the general. It is, in most cases, a dubious or false inferential method in that the premises do not deplete all of the complements or predicates inferred. Karl Popper, one of its most virulent critics, observes that the number of black crows observed is not important ; it does not follow that all crows are black (Popper 1978). In socio-cultural or socio-political fields, inductive arguments result from clichés, prejudices, feelings of hostility, racism, or other. What is true of some is not true of all.

The deductive argument, more in accordance with logical reason is an interference specifying from universal premises. The deduction concludes the truth of "all" from that of "some." The deductive argument is seen in two forms: "Modus Ponens" and "Modus Tollens."

Modus Ponens (literally: mode of asserting) concludes the consequent of a conditional premise if the antecedent of this is repeated as a simple propositional premise. In this form, at least one of the premises is a conditional and the other the repetition of its

antecedent. Example: If it rains, Yao goes to the fields; then it rains; therefore, Yao goes to the fields. In semi-formal language: If P then Q; so P; therefore Q

The *Modus Tollens* (mode of saying) concludes the negation from the antecedent of a conditional if its consequent in the position of premise is denied. If P then Q; or no Q; therefore, no P.

Thomas Jefferson said of argumentative activity: “In a republican nation whose citizens should be led by reason and persuasion and not by force, the art of argument turns out to be of the greatest importance;” and Juliana Geran Pilon: “Civilized life depends on the success of reason in social relations, the predominance of logic over violence in interpersonal conflicts” (cited by Copi: vii). In the same vein, M. Boll and J. Reinhart, in their *History of Logic* write:

The knowledge of at least rudiments of logic is recommended as one of the foundations of the true humanism of our time: excellent intellectual gymnastics, scientific logic is capable of clarifying confused thought, by banishing expressions with ambiguous meaning, by eliminating the vague “more pernicious than error.” Finally, by its very spirit, it warns against the paralogisms of affective origin and against ideological con games, which in our “Enlightenment,” continue to flourish at all levels of society (Boll, Reinhart 1961:9-46).

This intellectual gymnastics which is so indispensable is corrupted by specious arguments called sophisms. These are true epistemological obstacles which we should be aware of, and which reason – in the quest of true knowledge, even temporary or unfinished – should be warned about. I will only mention several examples of sophisms by way of illustration.

Sophistic Arguments

An argument or reasoning is a linguistic activity in which one wants to prove that a certain proposition, called a conclusion, follows or is meant to necessarily follow from data or reasons called premises. It targets the pragmatic function of language, provided that it convinces or persuades. It should then be valid and correct. An argument is valid when its conclusion follows logically from premises; it is correct when its premises and conclusion are materially true. The sophism claims to satisfy these conditions, but, upon analysis, we realize that it does not include any logical relevance; the conclusion does not follow from the premises.

The examination of linguistic functions has identified two cardinal values of language, namely the truth and its associated values on the one hand, and passions or desires on the other. The sophism results from shrewd, malicious, inappropriate combinations, carefully-maintained between feelings, attitudes and reason, a mixture of beliefs and desires with the aim of extracting belief or support. Logicians have attempted categorizations in terms of “sophisms of relevance” and “sophisms of equivocation” and “sophisms of vacuity” (see especially Copi, Fogelin et al.). I propose a classification which conforms to two basic functions of language. Epistemic sophisms and the sophisms of action, respectively, correspond to knowledge and action.

Sophisms of Action

Sophisms of action are related to fallacious arguments proposed to a listener to cause a non-linguistic behavior of a certain kind. This is a biased use of the directive function. The speaker can achieve his objective either by sweet-talking the listener or in forcing him – i.e. by appealing to his feelings: recourse to fear, intimidation, pity, enthusiasm, hostility, etc. is the arm the most frequently used to constrain action. What is psychologically relevant is taken for a logical relevance. The emotionally charged premises can be true, but upon analysis, we realize that they fail to serve as evidence for the conclusion. Certain sophisms are known by Latin names.

Argumentum ad baculum (appeal to force)

This sophism is based on the tacit or explicit use of intimidation, force or threat to elicit action. It thrives in the universe of political and social relations, relations based on force, and others. Example: “We have not signed your contract for the exploitation of the oilfield because you do not share our political opinions.” Even if this argument is an effective way to lead the economic operator to share the political opinions of the speaker, there is no logical link between the premise and the conclusion.

Argumentum ad misericordium (appeal to pity)

This sophism is committed by appealing to the pity, the mercy of the listener, to obtain a certain result. Our relationship with others bears the traces of this, using flattery. Some lazy students do not hesitate to resort to this. “Professor, sir, I need a C in your class. I realize I haven’t really done my best, but if I don’t have a passing grade, I’ll have to repeat the year; but then I will lose my scholarship. Yet, I am the only son of a poor mother chased from the house of her late husband by the traditional heirs.” Here, also, the truth of the premises does not imply that of the conclusion.

Argumentum ad populum (Appeal to the crowd)

This sophism appeals to emotion, to feeling, in order to lead the public to accept a conclusion: “As true Ivoirian patriots, the ones who put the interests of the nation before their petty advantages, realize, the international policies of conflict resolution in Côte d’Ivoire are policies of subjection and neo-colonialism in our dear country, a country of peace and fraternity; it then follows that these are bad policies.” The emotional call to the crowd may stir important feelings, certainly, but that has nothing to do with the truth of the conclusion.” Another example is the following: “I call on you to vote for our candidate because everyone in the region supports his program of government.”

The sophism of the slippery slope: A fallacious argument is called such if it maintains that an action would bring on a catastrophic situation because of a series of causes and effects which, upon examination, can prove to be dubious or avoidable. “You have to keep me in power since my removal will bring on civil war or chaos.”

Epistemic Sophisms

Sophisms of action, as their name indicates, raises this interrogative intentionality: “How do we encourage a particular listener to produce a particular action?” “How do we convince the consumer to buy a particular product?” Epistemic sophisms, those linked to knowledge, are in a different register. Here, we are attempting to understand or know, either by refuting or by establishing an argument.

Ignoratio Elenchi (Ignorance of the subject): This sophism is committed when a speaker establishes the truth of a conclusion by premises which have no relationship to it. “All children should receive the steady attention of parents. Parents who work full-time cannot provide this attention, thus mothers should not work full-time. The sophism of the hasty generalization: This consists of inferring general cases from specific cases. This is inductive reasoning: “All horses are white because we have seen some white ones.”

The false dilemma: This states that a given situation presents only two alternatives, one preferable to the other. There is a sophism if our examination shows other conclusions. “Either we disarm, or we fight the war. We do not disarm, therefore we fight the war.”

Ad hominem attacks: We criticize an interlocutor, in his physical appearance, in his person rather than his reasoning to claim to have thereby refuted his argument. “What Socrates said cannot be true because he is ugly.”

The double fault: This is committed when we justify bad actions compared to what others have already done. “You do not have the right to accuse us of poor management since you are not role models in the field.”

A sophistic argument may be committed for several reasons:

1. The calculated will to mislead: The speaker, in a contextual situation, can realize that convincing logical arguments will fail to get his conclusions accepted, and that the audience, uncultivated, not very shrewd, incapable of judgment, or simply a supporter of his cause needs nothing but to be served in one way or another.
2. Ignorance of the speaker: a person can commit a sophism without realizing it, either because he or she has no knowledge of the matter at hand, or because he or she is not sufficiently trained in the field. There is a lack of judgment.
3. Blindly forging ahead, way out. Sometimes a sophism is a means of weaseling one's way out because it is effective.

A sophism can convince or persuade a cultivated or uncultivated audience, thanks to racial, ideological, tribal, ethnic prejudices, etc. whereas for the same reasons, another sophism or the same may not be convincing.

In all sophisms, whether conscious or unconscious, its author never admits to lying, even when he knows that he is lying.

Logic, Argument and Proof

Upon reading the title of this section, we might be perplexed. Since logic is the combination of clauses by reasoning or argument and we have defined reasoning as an activity of proof, insofar as proving is the synonym of showing, isn't proof another name for argument and vice-versa? The distinction between proof and argument, although nuanced, deserves, nevertheless, to be maintained for a perspective on the nature of logic relatively to the formal sciences and sciences of man or ordinary language.

Logic, the science of the combination of clauses by reasoning invests two cognitive fields: the field of formal sciences or of nature and that of the sciences of man. It provides information to the formal sciences in the form of formal logic, and the second, most often, in the form of informal logic. What is formal logic? What is informal?

Formal logic is this operation of the mind which, from one abstraction to the next, empties the data of language and the physical world of their contents to retain only that their abstract forms, named by symbolic signs for the purpose of purely deductive calculations. Formal logic, like all informal logic, includes the logic of clauses and the logic of predicates, and can include bivalent logic (true and false) or plurivalent logic (deontical, modal, etc.) in the form of formalized systems with rules of construction and especially of mathematical proofs. In the logical proof, we know exactly the operations in play and the conditions which a series of clauses should satisfy. We will define proof, for example, as follows: "A series of clauses makes up a proof if and only if ..." This is not the case in argumentation. "To argue," writes Jean Blaise Grize, "is to display an activity which aims to weigh in on ideas, opinions, attitudes, feelings or behaviors of someone or of a group of people" (Grize 1996:5). There is, on the one hand, an intention to influence the listener since the purpose of the line of argument is to lead his listener to accept a conclusion. Argumentative logic is based on ontological assumptions of values and beliefs; the logical-mathematical approach can thus be applied without mutilating it. "That does not mean," Grize reassures us, "that things happen in any old way and it is legitimate: a) to try to find what are the operations that are the basis of all statements, and b) underline some of the procedures which connect them, i.e. what are the arguments in play" (Grize 1996:4).

In conclusion, I propose some procedures for constructing arguments for researchers. The suggestions given here are a summary of a work in English, *A Rulebook for Arguments* by Anthony Weston.

General rules for the construction of arguments

1. Identify premises and conclusions;
2. Present the ideas in a natural order;
3. Begin by relevant premises;
4. Use definite, concrete and specific language (avoiding using general, abstract, ambiguous and vague terms);

5. Avoid bombastic language (do not try to make one's argument good by caricaturizing the opposite argument);
6. Avoid sophistic arguments.

These rules apply to any deductive or inductive argument, each containing certain specificities. Here is a list of types of arguments that we may encounter in the field of social science research.

1. Arguments by examples: A construction of this sort offers one or several specific examples of evidence of a conclusion and raises the question of the sampling of examples, their representivity and the existence of counter-examples.
2. Arguments by analogy: The exercise here is not to multiply examples but to conclude from one example or case to another, provided that they are similar in one way or another and in a relevant way.
3. Arguments of authority: Often, we have to count on the account of others to know what we cannot know ourselves. We leave it to the opinion of those whose knowledge is authentic. The researcher should, however, ask if the authority or expert providing the information or knowledge is qualified or not, impartial or not. He is well advised to look for contradictory accounts in order to confirm or refute the information.
4. Arguments of the causal type: To explain an event, a case, and effect, we often look for the cause. Given the existence of relevant and non-relevant causes, the researcher should show a great deal of precaution, critical ability in the choice of explanatory causes.

When attempting to construct arguments, we must keep in mind that there are two cognitive paths out of three possibilities: 1. Knowing the premises (data, hypotheses, postulates), we must look for one or several conclusions. 2. Or the contrary: knowing the one or several conclusions, we must look for the seminal evidence, the premises. There is no knowledge at all: 3. In the absence of premise and conclusion.

If in the first two cases ignorance is partial or feigned or Socratic; it is complete and real in the latter: "Of nothing, we know nothing." René Descartes' approach is an example: hyperbolic or methodical doubt is shown to be an illusion. "Of nothing, we can know nothing." Holding forth on this maxim is nothing more than an inexpensive sophism.

Note

1. By logic, here, we do not mean that Africans, or Europeans, etc. each have their own way of reasoning or thinking. We would simply like to refer to the idea of differences of beliefs or values, in short, ontological assumptions.

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6

Construction of the Subject as a Practice of Clarification of Social Relationships

Jean Ferdinand Mbah

Social sciences, Luc Van Campenhoudt teaches us, “are not defined by the phenomena that they study: the phenomena called “economic” by economists, the phenomena called “social” (...) by sociologists or the phenomena called “political” by political scientists. They are defined, theoretically, by the perspective used to study phenomena, by the way in which they make phenomena the subjects of knowledge” (Van Campenhoudt 2001:48). And according to the authors of “The Profession of Sociologists,” sociological research “is organized, in fact, around constructed subjects which no longer have anything in common with the units dissected by naïve perception” (Bourdieu, Chamboredon and Passeron 1968:57). The immediate, the evident, experiences, are thus only “the surface effect of underlying mechanisms which are at work below (...). The immediate subject is only, at best, the first milestone from which the constituent elements of systems – or of structures – of explanation are articulated or understood” (Vidal 1971:19).

From this perspective, the sociological subject will only be completely defined by the assessment of the degree of deepening of the distance that the researcher will mark between the immediate subject and the constructed subject. There are not, *a priori*, good and bad subjects; there are not good and bad research subjects, as we can attest from these several examples taken from two (2004, 2005) recent methodological workshops organized by CODESRIA in Central Africa: “Hyper-religiosity and Destabilization of the Family in Kinshasa – Reading of an Existential Paradox”; “The Reappropriation of Endogenous Medical Knowledge in Kinshasa”; “Emergence, Politicization and Fragmentation of Feminist Movements in Kinshasa: A New Reading of Congolese Realities in a Crisis Situation.”

These specific subjects of study, as they have been presented and submitted to discussion, necessarily called for new questioning, not only on the various choices

made, the methods used and the concepts implemented, but especially on the social relationships to be better defined. In other words, it was not simply a question of trying to understand the social relationship such as the social actors were experiencing or understanding it; it was rather a question of elucidating, through these subjects, what was eluding these actors and could not help but be missed by them. It is in this perspective that it is necessary to adopt a true scientific approach, that which according to Patrick Champagne, “implies reflexivity, a break with common sense and especially the work of construction of a problem” (Champagne 1990:167) likely to lead to the clarification of the texture of social relationships.

These epistemological preconditions, which characterize research, remind us that the subject of knowledge “is never given in advance, never established in a univocal way: it is always constructed” (Campenhout 2001:48), and registered in a given issue or as advocates of qualitative research explain, in an issue which will evolve with the familiarity that the researcher will have little by little constructed with his corpus after a first reading of the data (Paillé, Mucchielli 2003:16).

In African sociological production, fairly broadly influenced by a dualistic understanding of social phenomenon, work often supports the description and interpretation of social facts on bipolar concepts such as: colonized-decolonized, underdeveloped-developed, traditional society-modern society, rural-urban. The contradictions and conflicts only appear at the first stage of research when inequalities, hierarchies, i.e. dualisms have to be pointed out. After which, the analysis tends to concentrate on and be limited to internal relationships of the sub-system (traditional society), as if this could still be envisaged as a significant totality. Yet, what is in play are economic, political and cultural relationships which characterize, maintain and develop the internal inequalities and disequilibrium in these dependant social formations, more and more integrated into the world economic system.

If we raise the issue of the analysis of social phenomenon in terms of structural dependence, the theoretical formulation and the practical implications of the questions undergo essential transformations. However, the sociological issue of the analysis of social then assumes the renunciation of synchronic and functionalist perspectives, which, as foundations of the “dualist” analysis of subjects, envisage their study while ignoring time, i.e., by searching for a way to explain the present in the present. In these conditions, “we cannot get around the task of construction of the subject without abandoning the search for these pre-fabricated subjects, social facts dissected, perceived and named by spontaneous sociology or “social problems” the claim of which to exist as sociological problems is all the greater because they no longer have social reality for the community of sociologists” (Bourdieu, Chamboredon and Passeron 1968:60).

The subject to be constructed in this text is tribalism, defined in the context of construction of the post-independent African state. The facts and discourse¹ which render an account of this, are excerpts of the Gabonese social group where the phenomenon, revealing of recurrent political and social tension on the issue of power, has often given rise to approximate and consensual approaches directed both towards the accusation of tribalism and towards the search for ethnic equality and equilibrium. In attempting to become familiar with it, social practices in which and by which the reproduction of the social group happens are of interest to us, to the extent that they fix class inequalities as relevant mechanisms for the domination of one over the others. Its construction authorizes us to go back in time to explore the situation of the phenomenon in Africa from which the method will be derived which will guide the process of production of the subject.

In 1958, the Pan African Conference of Peoples (Accra Conference of 5-13 December 1958, denounced tribalism as a “demonic practice” and as “a serious threat (to) unity... (to) political evolution (and to) the rapid liberation of Africa” (Bozon 1967:862). Roland Olivier and Antony Atmore had written that despite colonial domination “the potential of force of tribalism was still greater than nationalism in many regions. Over the entire continent, regional or local interests based on tribal or linguistic groups were threatening the security and stability of the new States” (Olivier, Atmore 1970:299).

The understanding of tribalism is thus not something immediate, because this phenomenon encompasses very different realities: the use of the term “tribalism” is itself very derogatory and deprecating, and the same is true of the problems that it raises, particularly hate, opposition, struggle, explosions, which connote a confused and opaque semantic field (Copans 1990a:950). Over the past four decades, it has taken over a number of disciplines (anthropology, sociology, political science), their theoretical boundaries as well as their common ground. A recurrent key word in “IPOs” launched by scientific institutions or jargon of political statements of all sorts, the word tribalism will have a tendency to lose its content through an excess of meaning. We will speak of threat to national unity, of centrifugal force, of self-awareness of an ethnic group or of a new dynamic of the tradition which “liberates the forces contained during the colonial period, as we see in several crises that have occurred during recent years which show the resurgence of tribal and/or religious antagonisms” (Balandier 1967:207). Georges Balandier underlines that “modern political activity has only been able to get organized and to express itself by resorting to a true translation; traditional models and symbols once again become the means of communication, and explanation, which officials speaking to peasants have recourse to” (Balandier 1967:5) and more broadly to the whole of post-independent society. In his article, “Tradition and Political Modernity in Africa,” Luc de Heusch explains that “the term tribalism designates at least two orders of more or less distinct reality: more or less strong

acuity of ethnic oppositions in the rural milieu. This factor is not necessarily traditional; the expression “tribalism” can also designate a certain urban “ethnicity,” also foreign to the classical approach of social and cultural anthropology. This time it means regrouping into neighborhoods, within spontaneous socio-economic associations, of people belonging to the same ethnicity, either to a super-ethnicity, or even to a foreign country...”²

According to Sylla Lancine, tribalism is rather “a behavior, a positive or negative attitude which creates, in a given social milieu, a network of attractions and repulsions between the members of two or several groups composing this social milieu. The members of each of these groups claim to be linked by blood, but they are much more so by the idea that they have of themselves in relationship to others. Moreover, this tribalism is a group mentality, a gregarious illusion or a state of mind which determines the conduct of individuals belonging to a same group and which regulates their relations, often aggressive, with members of similar groups. This group, which presents itself as opposing others and whose members believe that they are linked by blood, is the tribe” (Lancine 1977:27).

For Guy Landry Hazoumé, this concept designates “the solidarity of the ethnic or linguistic group... We use the term tribalism instead of that of ethnocentrism” (Hazoumé 1972:26-27).

Thus, although there no longer exists a “traditional” Africa strictly speaking, “so true it is that Islamic or Christian values and the ideas forces of Western civilization have brought profound perturbations to the most distance places, affecting more or less, depending on the case, structures (institutions, beliefs), behaviors, mentalities (Thomas, Luneau 1975:266), the mode of phenomenological knowledge which underlies the new dynamic of tradition inspired by Balandier and from which tribalism results, does not raise any question on the relationships which structure the observed facts. On the contrary, things “go without saying.” It is, on the other hand, the frequency of tribalism which will give way to the constitution of a social problem which becomes evident to all and is grafted onto a dualist conception, sometimes explicit, sometimes implicit, according to which development, thus modernity, would allow us to absorb ethnic resentment and would create the conditions for the eradication of tribalism.

Whether this be the pre-independence period (1958-1960) or the post-independence period (1960-2010), transparency is such that it makes the construction of the subject imperative. This essential phase of research consists in cutting a sector of reality, i.e. selecting certain aspects of this multiform reality and “discovering behind common language and appearances, inside a global society, social facts linked by a system of relations particular to the sector studied” (Bachelard 1968:17). The construction of the subject requires recourse to the method. On this plan, “the dialectical is the most complete, the richest and (...)

insofar as the most complete of methods leading to explanation in sociology” (Grawitz 1990:383). To the extent to which “the dialectical corresponds to fundamental requirements of the concept of method, “it is first an attitude vis-à-vis the subject: empirical and deductive, it therefore calls for a certain way of collecting concrete data. It then represents an attempt at explanation of social facts, i.e. it is directly linked to the idea of totality” (Grawitz 1990:384).

After the choice of the method follows, of course, its *exposé*:

“Dialectical logic requires that we go further. To really know the subject, it is necessary to embrace and study all of its aspects, its relations and “mediations.” We will never get there entirely, but the necessity of considering all aspects protects us from errors and torpor. There is the first point. Secondly, dialectical logic requires that we consider the subject in its development, its “particular movement” (...) its change (...) its relation with the external world (...). Thirdly, all practices of man should enter into the complete “definition” of the subject, the relation of the subject with what is necessary for man. Fourth point: dialectical logic teaches that “there is no abstract truth,” that “the truth is always concrete” (Lenin 1960:94).

This chapter is aimed at defining the scientific practice of the researcher confronted with the difficult problem of the construction of the subject of knowledge, a subject which is here always the expression of a relationship of force, on two accounts. First, because it necessarily renders an account of a conflictual relationship which is not obvious. Next, because the researcher himself has the chance during observation, “to enter into a relationship with his subject which, as a social relation in a relation, is never of pure knowledge, the data present themselves to him as living, singular configurations and, in short, too human, which tend to establish themselves as structures of the subject “(Bourdieu et al. 1968:36). Finally, it is necessary, in order to construct the subject, to break with “the real and the configurations that it proposes to perception”³ because “in sociology as elsewhere, serious research leads to reuniting what the ordinary separates or distinguishing what the ordinary confuses” (Bourdieu et al. 1968:7). The construction of the subject can be divided into three subjects: immediate, pre-constructed subject; then analogous, quasi-constructed subject; and finally constructed subject of knowledge.

The Materiality of Tribalism as an Immediate Social Fact

Today, ethnicity, after having given a negative image of society, would seem to be a positive element which would lead social actors to foresee a new form of sociability going forward. In other words, despite the deconstruction of the concept of ethnicity by Jean Loup Amselle and Elikia Mbokolo (Mbokolo, Amselle

1985), it is considering the dimension of the new sociability, aimed at producing a change in ways of acting, which begins a different construction of the concept of ethnicity.

By accepting that for the following reasons: “acceleration of urban immigration (...), failure of the class wars, abortion of the process of formation of a group of proletariat or a peasantry responsible for revolutionary hopes, questioning of certain aspects of national or nationalist ideology, ethnicity has become a positive value of identity” (Taylor 1991:244), and that “henceforth defined from the interior (...), the ethnic consciousness would take over from the class consciousness which history has not allowed to emerge” (Taylor 1991:244), it would then establish itself as a social fact.

If as a positive value, ethnicity can lead to induce active research on the process of adjustment and integration, it would begin a new perspective on ethnic relations through conviviality. It is, at any rate, the direction of the Pan African Association of Anthropology which, during its workshop held from 1-4 September, 1997 in Yaoundé, Cameroon, stressed the necessity of limiting ethnic conflicts in Africa. The identification of ethnic consciousness to class consciousness raises another theoretical problem in that there is a risk of equating these two concepts in a reality that is one and the same. The transitional situation of the society indeed finds an equation between ethnicities and classes and this juxtaposition, despite studies on social classes already conducted on Africa (Afana 1966; Diop 1972; Nkrumah 1972; Agier, Copans and Morice 1987), from time to time returns ethnicity to the scientific scene as the element which would mark a turning point in the definition and the question of inter-individual and collective relationships, which then would no longer be treated with the driving force and the ill-considered prejudices of the “tribalist” spirit. This issue, in developing a new dimension of ethnicity, was to accentuate the consciousness of a false group, insofar as ethnicity despite its “ethnic minority,” “multi-ethnic society,” “ethnic majority” by-products is only a pre-concept or pre-idea which is used in a mechanical and ill-considered way. There would only be the dynamic aspect of ethnicity to direct the organization of social relationships. This approach, which aims to raise the ethnic question through conviviality, also leads current research to develop under the hegemony of the concept of ethnicity, which is aimed at overriding the former hegemony, the concept of development. On this level, the corrections between social structures and ideologies are translated clearly because it is not a matter of producing a scientific discourse but an ideological discourse on tribalism or development.

We then need to come back to the idea of ethnicity. But in this need to turn back, the tendency is great to turn away from the critique of this concept in order to direct creativity towards fields of research which, while wishing to rethink the total change in structures and schemes of thought, suggest models in agreement with the requirements of development advocated by the neo-colonial State. The definition and the delimitation of “ethnicities,” in their form, their content, and

their space, are leading today to a redistribution of ethnonyms and forms of speech. Such an undertaking, although not without relevance, is in no way attacking the refoundation of a concept which would render an account of what is happening in a social group dominated by relationships of capitalist production. It is rather as a fact of "tradition" that ethnicity was to support the construction of tribalism as a social fact referring to the self-awareness of the group, to the feeling of belonging and social and cultural identity, to the division by quotas of administrative and political jobs within the state apparatus. This enumeration clarifies the drift of ethnicism.

As an immediate subject, tribalism appears to be capturable by observation as a sum of representations: "starting from independence, the first concern (...) was to oppose centrifugal tendencies that the ethnic diversities were exerting so much (...)," "in order for peace to reign within our walls, it (...) is necessary to ignore (...) tribalism," "the party condemns the supremacy of one ethnicity over another."

These various expressions have a relationship with reality. Indeed, for the actors involved in political life and for no matter what observer, the ethnic equilibrium advocated, translated by the term "geopolitical," has created support because during each cabinet meeting, one of their own (parent, friend, fellow student) is promoted to a prestigious and enhancing function within the state apparatus. In many cases, this is both a request made by the governed and a response of the government in a game established by the government itself. The discourse of "governance" established in a certain way that in order to enumerate the ethnic groups to the State: "no ethnicity will be forgotten;" no region will be forgotten;" "no ethnicity is superior to another;" "no province is superior to another."

If this reality is not the scientific real, in order to go from the immediate, preconstructed subject to the scientific subject, we need to stand back, to create some distance with this "phenomenological knowledge," i.e. the "innate knowledge" that the members of a human group possess implicitly as to the multiple characteristics of the space where they live" (Lacoste-Dujardin 1976:116).

The epistemological problems here take on two aspects. First, it is necessary to deconstruct the illusory knowledge of common sense which results from this spontaneous approach of phenomenon. Indeed, thought should target the discourse already constituted (immediate subject) in which tribalism, as a label, is never strictly defined but has become an "all-purpose concept" rendering an account of both of the supposed conflictuality between ethnicities, between political parties with real or fictitious electoral bastions (each party being meant to represent an ethnicity), of the necessity of equilibrium and ethnic equality within a single party and also the state apparatus. Next, it is necessary to challenge its transparency to discern under the appearances the true problems which fuel the questioning. It is in this perspective that the reflection on the discourse to be

constructed, in initiating an approach which does not take up the social dissection social of the real coming from social problems of the moment previously mentioned, leads to the identification of a specific problem of research.

The Materiality of Tribalism as an Analogous Subject

To break with the immediate subject, it is necessary to start from two phenomenological situations. The first describes tribalism as a specter which disunites social groups and hinders development and social progress: "our fragile and recent unity has adversaries which are still strong like regionalism, tribalism;" "It is necessary to succeed at defeating our old familiar demons of regionalism and tribalism;" "let's put aside all our political histories which are rather tribal histories." The second evokes development in the following terms: "In order to prevent the flight of foreign capital without which the economic take-off of Gabon cannot occur," a "single party within which we see a permanent intermingling of persons and ideas from all (...) the ethnicities" has been created.⁴

In the order of priorities, development of the economy precedes the fight against tribalism and is even the determining factor for even the realization of national unity. It is thus an issue of "situational elements, described from the viewpoint of actors, forming organizations which are similar and synthesizable in a single situational form enabling us to achieve a global meaning experienced by the actors in the situation" (Mucchielli 1996:15). We can then consider from now on that tribalism has no particular specificity and that it is necessary to place it in relationship with development: "We forever banish this spirit of tribalism to make Gabon a modern and prosperous country;" "the market economy has (...) encouraged contacts between the various ethnicities;" "tribalism and nepotism, which is its by-product, present a threat to our regions which is infinitely more serious than under-development."⁵

The relationship between the two phenomena is very telling, because: either the discourse of the political class only takes up modified, reworked elements of the ideology of development of which tribalism presents one of the stumbling blocks, or their statements with respect to their bases are radically opposed to or differ from the presuppositions of the ideology of development. In the first case, this will only be a discourse of relative autonomy which shows how a social group, at a given period, "codes and decodes its experience of the world in a specific way which bears the mark of social relationships" (Flament, Rouquette 2003:11). In the second case, ideology will be specific and we will then need to examine not only its explicit components (development is progress, social well-being), but also those which are implicit (tradition in general, tribalism and blood relationships as by-products represent obstacles). But be careful, J. Copans warns:

"if there is a socio-intellectual practice which the African powers have been divested of, it is precisely thought on development, on their

development! And if this reflection does not exist, it is simply that development is something totally different from a sociological and economic transition. Development is a procedure of maintaining the status quo on the international scale which includes the extension of perverse relationships of reproduction” (Copans 1990b:160).

In other words, when the ruling political elite corners the concept of development as ideology (dominant), it creates a second one: tribalism. Thus, considering relationships social forces, the articulation in which the dominant discourse plays out is this relationship between what it targets and what is discernable in political language: a positive relationship first since development, which proposes social transformation, is led to an attentive examination of *de facto* conditions, i.e. a type of analysis of the real. A negative relationship since tribalism throws into question the effort which is undertaken. In fact, these two aspects (positive and negative) are more or less linked, the negative relationship determining the reading of real and the analysis of real intervening in the strategy of the dominant class as a form of critical description. With respect to development, it is led to explain its difficulties by tribalism, ending up finally by making it the enemy, then mythicizing it.

At this level of the presentation of the analogous subject, “discourses, acted or spoken (...), policies (...), are both obstacles and supporting points, rival discourses and part of the subject to analyze” (Champagne et al. 1990:160). These acted and spoken discourses of the field of observation appear as scholarly preconstructions of the main producers of speech and of a schema of society. When the researcher is placed in the situation “of analyzing a reality which presents itself to him, in more or less developed forms, a theatricalization: he should distance himself from this, but be able to render an account of it” (Champagne et al. 1990:160). But how do we break when “the sociologist should consider the fact that today there is a true ‘scholarly common sense,’ a sort of mixture of ordinary common sense and products of social sciences, insofar as (...) each year a number of sociological works appear (...), in short, since there has been a broad distribution and as a result a sort of popularization of the approach of human sciences or at least of its concepts and its results?” (Champagne et al 1990:165). This concern that Champagne expresses is at the core of the concerns of a number of beginning researchers who, confusing social reality and sociological reality, act as if it were enough to provide oneself with a subject endowed with social reality, to have at the same time a subject endowed with sociological reality. And, it is along with Bachelard that we can remember that: “science realizes its subjects without ever finding them completely made (...) it does not correspond to a world of describing, it corresponds to a world to be constructed” (Bachelard 1968:61). Finally, how to break with this “scholarly common sense,” mode of interpretation that the dominant class uses to analyze the social group and its

conflicts, when this system of interpretation of social facts becomes the mode of knowledge that the society itself takes on? The answer will depend on the personal equation of the researcher. Indeed, “each research theme includes a different subject and each construction should thus adapt to the subject to be constructed. It is probably the moment when the extent of training of the sociologist can be assessed, it is the moment especially where we see the intelligence and the contradictory qualities of the researcher: intuition, rigor, knowledge and imagination, sense of the real and of abstraction” (Grawitz 1993:330).

If the social basis of this “scholarly common sense” is the class which controls the state apparatus, we have seen, its discourse at best only names “the domains of practices that the common language proposed to the sociologist as a field of research, without this registry, which only obeys laws of daily intercomprehension, ever requiring the formulation of principles of definition or indicating an approach of explanatory reconstruction” (Passeron 1992:49). The researcher then should be particularly vigilant to dispel the illusion that the concepts of spontaneous sociology would enable us to directly understand the subject of study, which would have spontaneously offered itself up to observation and understood empirically. Next, he should remember, faced with this common sense, that there is only a sociology of unequal relationships and figures of difference. Finally, it is important in these conditions to call for the epistemological principle which requires the researcher to work towards the unmasking of the hidden. Also, even “if it is possible that a problem related to the social, economic or political life of a society is a problem of research (...) it can (...) happen that it cannot be the subject of research if reasons specific to the process, such as the absence of previous studies, the inappropriateness of instruments of research, inaccessible data, etc., prohibit such a study” (Macé 1997:16). This is why, Macé continues, “it is not incumbent on the researcher to formulate a problem according to its social, political or other relevance; it should not be seen as a research problem” (Macé 1997:16). In order to do this, “the only way to justify a work is to locate a lacuna in previous work dealing with the same subject, and from that point, fill this void” (Macé 1997:22).

The construction of the subject will thus be related to defining the social context itself, i.e. the social group that one can consider to be in a transitional situation and where the capitalist relationships structure the ways in which the social classes act. It is by relationship to this context that the discourse of “scholarly common sense” was established for all the actors and has become an element of the system of ideological influence. Here, the reflection on ideology is double : development, issue of the simulated colonial discontinuity and model of representations and avowed values on the one hand; tribalism, decoded for what it reveals as elements of pre-colonial and colonial (tradition) continuity, on the other, are read for what they protect and mask, namely social relationships of

classes. For the actors of governance, ideology is “a system of opinions which, being founded on a system of accepted values, determines the attitudes and behavior of men with respect to desired objectives of development of the society, social group or individual” (Schaff 1967:50). However, the perspective of colonial discontinuity simulated through development, makes simultaneously possible the production of a discourse on continuity as a response to that of discontinuity. It is in this sense that tribalism, immediate subject inserted into a set of phenomena (development, regionalism, government subsidies, political struggle) which depends on it and on which it depends, is transformed into an analogous subject since as ideology, it is: “decoded, read not for what it gives, but what it protects, it masks: Behind the obvious modulations, the reason for these modulations. This intention and this reason are also ideological, in that they justify the outlines of it. After open ideology, hidden ideology, issue and model of avowed representations and values” (Vidal 1971:21). This is why we cannot study ideological phenomena (tribalism and development) as systems of representations endowed with internal coherence and a power of justification without relating them to class membership and the domination of the ideas of the dominant class. It is in fact proved – the systematic recourse to ethnic fact as an explanatory principle from social contradictions reveals, in any case, an attempt at simplification of the complexity of a social phenomenon, the effects of which are perceptible on different levels: economic, political, social, cultural. Thus, the construction of the subject takes the form of a recapitulative approach which integrates social relationships at work in the social group: those which link the classes one to another; those that the ruling class constructs with and by the State in which it constitutes and legitimates its action. The concept of class refers here “to a theoretical reality, namely, a place in the sociological field where the researcher should mentally place himself to provide himself with all possibilities to arm descriptive questions and analogical hypotheses a methodical work of interpretation of the differentiation or social inequalities” (Passeron 1992:39). With the assistance of this concept, we place social relationships in relation to economic relationships on the one hand and political confrontations on the other.

We can now state that the sociological subject “would not be the immediate real, it would be, in a way, the analogous real. Analogous because it would not reconstruct the immediate real in the same terms by which the immediate real lends itself to naïve reading. But analogous also insofar as this reading could identify it without difficulty” (Vidal 1971:19). Hence, the analogous subject, ideology, quasi-subject, is the product of a specific work of reading which renders an account of the explicit and implicit contents of the double ideology of the elite (tribalism and development). It was found from the time when we determined the place of production of discourse and the social group which is the starter of it. Analogy systematizes “recourses to ‘similarities’ and aims to ‘make the strange

familiar and the familiar strange” (Paillé, Mucchielli 2003:63). The referent “social class” already announces the epistemological position which will envelope all later questioning on the issue and the formulation of hypotheses.

The Materiality of Tribalism as a Subject of Research and Observation

Tribalism and development are the products of relationships of production in effect in the post-independent Gabonese social group within which the break with the past is much less profound than we might think since the previous domination is not abolished but only modified. In this transitional situation, policy registers the fact of ethnic membership as a fact of a constituted social group, with an autonomous economic and political organization, whereas ethnicity is rather an ideological reality here. In the economic realm, the transition is characterized by an almost total absence of development even if the concept of development, element of restructuration of the social group, would like to see tribalism as the explanation of its failure: it is the superstructure engendered by the economic structures of the situation of transition. The political class needed to construct, with a view to appropriating it for itself, the social and economic reality that it was experiencing, in order to confront it, dominate it, put up with it. But especially, it needed a justification which makes its objectives recognized as desirable; in short, an ideology. Yet, “the ideology that a ruling class makes dominant in its ideological state apparatus (ISA) is indeed realized in these ISA, but it goes beyond them, because it comes from elsewhere “(Althusser 1970:38), because ideologies are “not born in the ISA, but from social classes caught in the class struggle: from their conditions of existence, from their practices (...) (Althusser 1970:38), Social reality, constituted both of “true” and of “false”, the false being the illusion instituted and the true referring back to the allusion to the masked real, is thus not transparent to individuals. And, if it seems so, it is a necessary illusion produced by social relationships in order to enable the functioning and reproduction of the social group.

The materiality of the sociological subject begins here with the rupture which consists in detaching ourselves from the share of illusion in social reality, whereas the construction will be based on the allusion to the real that must be emphasized in identifying the research problem. This will be done successively through the critique of the concepts of tribe and ethnicity on the one hand, and through social representations of the phenomenon in the process of domination which enables us “to reproduce the dominators in their domination and subjugate the dominated” (Beynier, Le Gall, Moreau de Bellaing 1984:23). At this decisive moment, there is unquestionably, and in a completely direct way, “overlapping between (our) theory and (its) subject since the latter (subject of study) is only a construction of the theory” (Beynier, Le Gall, Moreau de Bellaing 1984:13). The critique of the concepts of tribe and ethnicity appear as the first act of an indis-

pensable construction because “the words of usual language, like the concepts that they express, are always ambiguous and the scholar who uses them as he receives them from usage and without subjecting them to other development would be exposed to more serious confusion” (Durkheim 1983:1).

The concept of tribe refers to a “homogeneous group, politically and socially autonomous, supposed to descend from a single ancestor by unilinear filiation, organized according to a segmentary mode and occupying a particular territory (...) or, on the contrary, a sub-unit of ethnicity (...)” (Gresles, Perrin, Panoff, Tripier 1990:336-337). One model of society among other, the specific mode of social organization that anthropologists compare to other modes of organization of society, “bands,” “States,” the use of the term tribe poses a problem.

For Maurice Godelier, “it does not suffice (...) to remain silent on the concept of tribe and to no longer invoke it, to appeal to prudence (...) or to criticize vehemently its scandalous lack of precision (...), its sterility and its theoretical untruth (...) and the ideological manipulations of which it is the instrument in the hands of colonial powers (...). But the “difficulties of the empirical concept of tribe come from elsewhere and insist, it seems, that this “general form” in which the social relationships typical of certain societies appear, does not only show the appearance of these social relationships but at the same time suggests something concerning their essence, their nature and their internal connections or, at least, of the fact that it does not make these social relationships appear only as aspects of blood ties, it prevents from seeing otherwise what it shows and to see something other than what it shows” (Godelier 1977:188-235). Today still, the use of the concept of tribe continues to pose the same problems, both for anthropologists and sociologists.

The second term of the conceptual critique is ethnicity, defined in anthropological literature as a group of persons who have perpetuated themselves biologically to a large degree, have fundamental cultural values in common, realized in cultural forms having a clear unity, constitutes a space of communication and interaction, is composed of a group of members which identify themselves and are identified by others as constituting a category that one can distinguish from other categories of the same type” (Poutignat, Streiff-Fernat 1995:206; see also Rohan-Csermak 1990:992-994). This definition gives rise to some questions: “Is ethnicity defined by the common origin of its members? Is this a culturally homogeneous unit? Is this a linguistically homogeneous unit? Is this a unit of lifestyle? Is this a politically organized unit, or at least a set inside of which the cooperation between the composing elements is intense and constant?” (Mercier 1961:65).

Schematically, writes Paul Mercier, “the ethnic group was presented as a closed group, descending from a common ancestor or, more generally, having a same origin, possessing a homogeneous culture and speaking a common language; another

characteristics was added, but not always: a group constituting a unit of a political nature" (Mercier 1966:65). On this latter aspect, Guy Nicolas believes that the idea of ethnicity "concerns a social equality which is situated on the sidelines of politics, where emphasis is placed on the cultural aspect by those who use it, whereas the 'tribal' group is presented as fundamentally political" (Nicolas 1973:113). However, what is important to note can be summarized in this assertion of Jean Loup Amselle: "the late opposition and specification of the terms "tribe" and "ethnicity" lead us to pose the problem of the congruence between a historical period (marked by European domination over the rest of the planet) and the use of these concepts. If these terms have taken on a general use, to the detriment of other words like "nation," it is probably that it was a matter of classifying some societies on the side in denying them a specific quality. This quality, the absence of which would make them dissimilar and inferior to our own societies, is historicity and in this sense the concepts of "ethnicity" and "tribe" are linked to other distinctions by which the large division between anthropology and sociology was made: society without history/society with history, pre-industrial society/industrial society, society without writing/society with writing, community/society" (Amselle 1990:971).

Beyond the myths, the role of which is, according to Roland Barthes, to transform social facts into facts of nature, and beyond the performative effects of nomination, "the emergence of ethnic groups, the mobilization of ethnicity can only be understood (...), as a function of social processes which are more all-embracing, or the expression of capitalism (...) and growth in government domination: increased penetration of the state apparatus, dislocation of civil society, erosion of the social fabric and former forms of sociability" (Juteau 1999:83).

The conceptual critique which has just been made of common language and of the ideological charge of these two ideas (tribe and ethnicity) in the Gabonese social context does not simply concern the illustrative value of the phenomenon of tribalism, but touches directly on its preliminary value which is much more subject to caution. Indeed, these terms which are widely used in social practice, refer "probably to (...) 'definitions of things,' but it is these very definitions which organize the preconstructions of spontaneous sociology, to which the mechanical use of such concepts refers, betrays its membership, in that it is hardly answerable except as a "deixique" definition, i.e. a nomination supposing the designation by the finger or the look of "what everyone calls that" (Passeron 1991:50).

The definition of the subject, by including the critique of language, narrowly draws the link with the general context of the subject and shows how incriminated terms and their recurrence become embedded in the practices of social actors (dominants and dominated). The effectiveness of this political discourse was thus able to be developed starting from a distribution of the term tribalism, tribalist and ethnicity, ethnic.

The first register of this distribution is economic: "The market economy (...) facilitated contacts between the various ethnicities;" "Tribalism brings to bear an infinitely more serious threat than under-development on our regions;" "Concerned (...) with attracting foreign capital without which the economic kick start of Gabon cannot be achieved, the single party was created to put an end to multipartism;" "Didn't certain ethnic groups or at least certain leaders advocate or haven't they advocated oppression of certain ethnic minorities in order to acquire with less effort economic 'better'-being?"⁶

The second register is political: "In order for peace to reign within our walls (...) it is necessary to overlook (...) tribalism;" "this rather somber tableau of the state of our unity should be considered as a warning against the bad Gabonese retrogrades who want to continue to react in ethnic and tribalist terms;" "inter-personal quarrels will always be closely watched by clever schemers and upstarts who will always be able more or less long-term to make them more or less degenerate into quarrels and inter-ethnic rivalries;" "tireless action was conducted (...) in order to avoid having ethnic favoritism damage the equilibrium of our national construction;" "This politician's policy never manages to give birth to a constructive opposition, but rather discord, the strengthening of ethnic barriers;" "political parties have a strong tendency to identify with an ethnic group. The rivalries between politicians always end up degenerating into inter-ethnic rivalries."⁷

Finally, the third register, the social, is less dense: "Members of Parliament, you will be the apostles of peace within all families, of peace between all ethnicities;" "No ethnicity can prevail over others;" "No ethnicity will be forgotten."⁸ This very slight corpus cannot be used here as a basis of analysis of formal content. It responds, above all, to our concern for emphasizing the approach more than the results. Thus, this corpus simply translates economic, political and social relationships between the lexical field and the various classes of society. Furthermore, it shows the link between the lexical field and the field of political experience, and especially how these words can become those of structures, especially political, and produce the expected effects. The government appointed after the Gabonese presidential elections of 25 and 27 November 2005 confirms the effectiveness postulated above in clearly marking the topicality of the real hold of the ethnic referent. This is the wording of an invitation: "following the nomination of their daughter, niece, and sister (...) to the position of Minister (...), the *esa mesila*, *ebemeko*, *essa me waba*, *ebifa*, *ebindzum*, *ebingum*, *ebifangli*, *ebindone*, *essibang* tribes and clans (...) invite (...) to the event of (...) that they have organized on Saturday, March 4, 2006."⁹

The location and effectiveness and of these discourses and practices obviously situated the dominant position of the organizing elite of the government, in part responsible for the massive distribution of the feeling of ethnic membership as a mode of participation in politics. On the other hand, insofar as the quantity of

information provided by each of the terms of the corpus is inversely proportional to the breadth of the semantic field it covers, we note, for example, that in the political discourse consulted, and based on which this fragment of the corpus was developed, the frequency of the word *tribe* (13 times) seems low compared to *ethnicity* (39 times) and almost equal to *tribalism* (14 times). And even if the effects of discourse win out over the relative redundancy of words used, it is because of their pejorative connotation that we will not accept them in the materiality of the subject – tribalism. Because it occupies a front and center place on the social scene going forward, tribalism no longer concerns simply the specific identity of an ethnic group; it intersects all the mechanisms of social and economic exclusion that the social group can experience. Indeed, since the process of construction of the subject marked, with conceptual criticism, the passage of the analogous, semi-constructed subject, to the subject to be constructed as a unit of observation, it seems more and more obvious that political life cannot be lived in the form of identity-based conflict (*ethnic*). At this stage, we will accept with Bourdieu and Passeron that “a subject of research no matter how partial and fragmented it may be can only be defined and constructed as a function of a theoretical problem which allows us to subject to a systematic questioning the aspects of reality related by the question asked of them” (Bourdieu, Passeron 1968:61-62).

To understand tribalism as ideology results from a process which is “produced in limited strata and groups of individuals (intellectuals, lawyers, politicians, etc.)” (Vadee 1973:6) and implies not only a relationship of power and of domination, but also a sustained problem which clearly distances itself from the ambient positivism. Michel Paty, citing Paul Langevin, writes: “the positivist attitude is essentially critical, analytical, and static; is more appropriate to draw up the check-list of acquired knowledge, to clearly formulate the structure and content of this knowledge than to show to way to extend it or renew it, more appropriate to note the difficulties than to resolve them. This allows of the elimination of concepts or theories, the denunciation of problems and affirmations devoid of meaning, but it does not allow for the formulation of directions for the construction of concepts or new theories” (Paty 1985:897).

The political nature of ideology which we see here shows to what extent our problem suggests establishing a special link between ideology and politics. Indeed, “an ideology is then only the expression, in the vast range of forms of language, of conscience and thought, of the situation of a class” (Vadee 1973:7). The problem is then directed toward the proposal of a functional definition of ideology, and underlines “the functions filled by ideology with respect to society, social groups and individuals” (Schaff 1967:50). If tribalist ideology “tends in part to hide social relationships (dominant, dominated) (...), if it presents the social organization as a quasi-harmonious totality and as the only rational one today, thus as “historical-

natural,” this is because it participates in the conditions of reproduction of system by presenting this type of social organization as the most optimal for all, whereas it is the project of a social fraction. The interests of a minority are understood as the interests of the whole” (Beynier et al. 1984:23).

A fact arising from the ideological practice of a class composed of “introducers of social disorder and guarantor of a future order” (Vidal 1971:21), the problem dealt with here implies two operations of detachment: the position of problems and the formulation of hypotheses. The main question on which the problem of research is based is the following: why has the political expression of the state not taken the social classes as referents since independence? As is often said, “science does not begin with facts and hypotheses, but with a specific problem” (Grawitz 1993:33), a problem which is truly the expression of the underlying social relationship to the subject. As we have previously stressed, this is an issue of a relationship of power, conflictual, between social classes which is not at all obvious and which is carefully masked by the ethnic referent.

Following the formulation of the problem, tribalism appears to be formed through two successive mediations. The first is based on elements of the “colonial situation” which, for lack of having been sufficiently understood as a situation of economic, political and cultural domination, independent of ethnic and regional membership, gives the elite who organize the government the chance to integrate the fact of ethnicity as the means of making up groups dominated by main actors in political and social life. The second mediation, on the other hand, is done by coupling tribalism with development. On the one hand, we promote the arguments of economic development, and, on the other, arguments of “local democracy”: a double strategy which first allows us to situate development in an established institutional framework (the state). Secondly, the issue is to displace the site of social struggles by situating them in a defined socio-cultural space (ethnicity), this displacement having the tendency to unload the possible unpleasant aftertaste of development on identified actors.

Considering that the ideology of tribalism, just as that of development, are dominant in the various state apparatuses, the mode of development-construction of tribalism becomes perceptible starting with the conception of all of the political elite in close relation with the question posed. But this question itself is the result of a theoretical effort. Theory, understood as “a more or less articulated set of implicit or explicit statements on a phenomenon subject to examination” or “a theory is a statement about the relationship of other statements” (Paillé, Mucchielli 2003:37) is well presented from the start. Indeed, if sociological observation should be “captured” at the expense of common sense, we should immediately add that observation and systematic analysis are often simultaneous and closely intertwined (Loubet Del Baye 1991:30).

The response to the question posed is articulated in a hypothesis: the political elite of independence, engaged in an effort of social transformation, constructed the post-colonial state by means of tribalist ideology and development. At this level, the hypothesis maintains its character of "doubtful but likely conjecture by which imagination anticipates knowledge" (Carbonnel 1996:168). Subsequently, considering it as an anticipated response which takes the form of a "temporary diagnosis," the hypothesis defines a world vision, a system of representations of a social group which ensures its cohesion, perpetuation and legitimizes a given form of social relations and modes of domination.

This hypothesis, which was developed after a patient work of construction of the a subject, is deduced from an already formulated theory of ideology, and which we can state as follows: "Dominant thoughts are nothing but the ideal expression of dominant material relations understood in the form of ideas, thus the expression of relationships which make a class the dominant class; i.e., these are the ideas of its domination" (Marx 1968:76). The theory allows us to consider that the ethnic referent has had the function of hiding realities engendered by class distinction. In this way, the concept of class represents a relatively ambiguous reality, both as theoretical concept (referring to the capitalist mode of production and its progression) which englobes a theory of ideologies, of social difference in general, of class struggle in particular, and as operative concept (participating going forward in the weakening of former social formations, in the life of class conflicts as an image expressing the violence of individuals torn from ethnic solidarity) which shows that there are also other concrete modes of representation of social relationships. This is why tribalism does not express class interests in a dissimulated way. On the contrary, it "constitutes a form of social affiliation in competition with class, the ideological function of which is to mask the class interests convergent between the ethnically dominated groups and the exploited fraction of the ethnically dominated group" (Poutignat 1995:118-119).

At the end of the construction, the temporary definition of the subject is the following: tribalism designated the intellectual content of concept, of images, of convictions, of assessments characteristics of middle-class and lower middle-class groups which are reinforced in the consciousness of dominated social classes and strata by a mutual suggestion, by the belief that they are also shared by all classes of the social group.

Conclusion

Tribalism is a phenomenon which, when it is recognized as global and absolute, i.e. as the true essence of the identity of a group, this right to be different then becomes a principle which can break up the state. Indeed, ethnic differences insofar as they are taken as parameters of political and social management, hinder

the functioning of the social group. In seeking a compromise between individuals and groups, the rationality of the state which is thought to function based on ethnic equilibrium and government subsidies designed to capture votes, may end up putting its own legitimacy at stake. At this point, we should recognize the relationship of tribalism to the State. Christian Coulon writes: “ethnicity, which is often presented as the explanatory key to political phenomena, is less a given acting on and imprisoning the political and the State than one of the effects of its construction (...). The ethnic phenomena (...) are founded in the genesis of the contemporary state (...). Ethnicity expresses the gestation of the state and uncertainties which go hand in hand with it” (Coulon 1998:51).

Taking the critique of language as a starting point, we have gone on to situate each of the words used by groups and individuals, because words are often misleading and refer to what Bachelard calls counter-thoughts, i.e. preconceived ideas, prejudices, false evidence which, left uncriticized, run the risk of unconsciously guiding research. To this first linguistic obstacle is added a second, the familiarity with the social universe, which constitutes for the sociologist “the epistemological obstacle par excellence, because it continually produces fictitious conceptions or systematizations at the same time as the conditions of their credibility” (Bourdieu, Chamboredon, Passeron 1968:35).

The social universe in which we have registered the construction of the subject – tribalism – is characterized by the presence of classic epistemological obstacles previously noted, for which the rupture is organized in strictly applying the hierarchy of three epistemological acts (conquest, construction and observation) knowing full well that this epistemological hierarchy of scientific acts “subordinates observation to construction and construction to rupture” (Bourdieu and al. 1968:31).

If this epistemological conformity presents the scientific approach followed, it involves more reflexivity from the moment when the empowered discourse takes on the appearance of more sophisticated discourse, thus taking the form of “scholarly common sense.” This characteristic of the social universe shows to what extent “the acquired knowledge of sociology tends to pass progressively into the social world and become part of the very functioning of society” (Champagne et al. 1990:166). Thus, the work of rupture with these various “common senses” (ordinary and scholarly) has led theoretical reflection to “contrast the systematic claims of spontaneous sociology to the organized resistance of a theory of the knowledge of the social, the principles of which contradict point by point the presuppositions of the primary philosophy of the social” (Bourdieu and al. 1968:37).

The reflexive attitude on the concept of ideology, taken as “a set of forms of by-passed consciousness which emanate from relations of domination of class and hide them” (Vakaloulis 1996:67) has allowed us to decipher the dominant

discourse. It is indeed the theme of domination which was at the core of the construction of the subject – tribalism – as the clarification of social relationship dissimulated by the immediate subject. The process of production of the subject has thus shown how, from the pre-scientific representation of the phenomenon having necessitated the rupture (logical critique of concepts, contestation of appearances of discourse instituted), we have arrived at the construction of the subject of knowledge via the analogous subject, thanks to concepts of class, domination, and ideology. These various concepts have contributed to state a hypothesis related to relationships of force between classes.

Notes

1. This is discourse of the political class.
2. L. De Heusch : “Tradition et modernité politique en Afrique”, *Cahiers internationaux de sociologie*, vol XLIV, 19 pp. 64-65. [“Tradition and Political Modernity in Africa”], [International Notebooks of Sociology]. Balandier has analyzed the phenomenon very well among black inhabitants of Brazzaville. In Christopher John Gray’s thesis: “Colonial Rule and Crisis in Equatorial Africa : Southern Gabon” which Roland Pourtier reviewed in the *Cahiers d’Etudes Africaines* [Notebooks of African Studies] XLVI (1), 181, 2006, pp. 205-209, the phenomenon of ethnic regrouping is touched on. According to Pourtier, C. J. Gray’s thesis affirms this opposition between, on the one hand, a world constructed on clanic entities, on their alliances formalized by the exchange of women, on the functioning mode on the mode of networks and spatial fluidity, and, on the other, a social organization based on spatial specificity and a territorial supervision, leaving no room for indecision (p. 207). The fluid and aterritorial system of clans has given way to spatialized ethnic categories, at the same time that the fluidity of space is reabsorbed by crystallization around its new fixed points – cities – and under the effect of rigidities brought about by the administrative conscription of territory (p. 209).
3. Ibid.
4. These are excerpts of Discourse of the Political Class.
5. These are excerpts of Discourse of the Political Class.
6. These are excerpts of Discourse of the Political Class.
7. These are excerpts of Discourse of the Political Class.
8. These are excerpts of Discourse of the Political Class.
9. Each minister named organized a celebratory ceremony to celebrate his “brilliant” promotion which was obviously that of his clan, his family, his village, his ethnicity, his province. These celebrations provide evidence of the way in which the system has succeeded in organizing social relationships around individuals made up, above all, of clans, families.

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7

Moroccan Sociology: Epistemological Preliminaries

Abderrahman El-Maliki

The researcher interested in sociology is, after a certain period of investigations and reflections, undeniably faced with a series of interrogations and questions of an epistemological nature which shout out to him and which are based on his scientific practice and the status of this science in his country. These interrogations and questions are gaining more and more legitimacy and relevance today after the birth and proliferation of the so-called “national” sociologies. It is in this framework that we should ask ourselves whether a so-called “Moroccan” sociology exists.

As sociology has become a plural science, it would be futile and even imprudent to want to return it to a single discipline (Balandier 1981). The ramifications and divisions which are ongoing within this discipline mean that it can only be conceived of today as the corpus of an infinity of sociologies: political, religious, economic, rural, urban, etc., but also French, English, Mexican, Indian, etc., according to the fields and diverse cultural realms that it takes as its subject. Nevertheless, within this plurality, there are specificities which result in each of these sociologies particularizing by method, subject, or purpose.

How then Can We Describe Moroccan Sociology?

In our opinion, it can only be characterized by the “national” ends and strategies which it makes its own. So when the native sociologist considers creating his own sociology, he is faced with a rich and cumbersome sociological literature: such a work is one by foreigners of which he is obliged to make the inventory and critique, in view of disentangling the scientific from the ideological. This task which first falls on the national sociologist, places him or her in front of the thorny problem of objectivity in sociology, a problem which still arises for both national and foreign researchers.

From the Sociology of “The Other on Oneself” to the Sociology of “Oneself”

The fundamental problem with which the national sociologist is confronted when he considers the study of a social phenomenon is first his dependence and familiarity with the phenomenon, which could at times lead him to consider it as one of the daily trivial matters. And yet the everyday sometimes conceals the real, because “what is familiar is not necessarily known,” said Hegel.

The national researcher, submerged by the social phenomenon that he is dealing with runs the risk of coming up short with respect to the exteriority guarantor of objectivity required by the “scientificity” of the 19th century. How, then, can a native make his own anthropology, some ethnologists asked themselves? Is it possible from the perspective of the social sciences to be object and subject at the same time?

To attempt to provide some answers to these questions, we will content ourselves in the following pages with drawing up an outline of the “identity” of the sociologists who have been interested in Morocco, and we will then see the difficulties which lead to the fact that objectivity in sociology – the example of which is borrowed from physics – still remains a subject of controversies and all-out debates.

Sociologists who have examined the history of sociology in Morocco have agreed to divide it into two main periods:

- sociology of the colonial period;
- sociology post-independence.

Sociology of the colonial period

This sociology was the work of foreign researchers – mainly French – and had as its main objective the knowledge of Morocco, all the better to dominate it. With this backdrop came the establishment of the “Morocco Scientific Mission,” a research entity established in 1903 in Tangiers by Alfred le Chatelier, well before the creation of the protectorate in 1912.

In 1913, the role of the scientific mission was made official by an order of the first resident general in Morocco, General Lyautey. We can read in this decree:

The notices established in the various regions on the ethnographic, historical, sociological, economic and administrative of cities and tribes in Morocco, and the other work of agents of the protectorate on indigenous sociology or politics will be made available to the Morocco scientific mission, responsible particularly for the preparation of a documentary collection published under the auspices of the general residence (Nicolas 1961:187).

The establishment of this mission was also a part of the “new positivist confidence in social analyses (which) made a scientifically-based colonialism (pacifistic and inexpensive) possible” (Burke II 1979). Thus, colonization of Morocco “was to take place, according to the protagonists of colonizing action, not by force and armed occupation but in a “pacifistic” and “scientific” way” (Halim 2004:6). As a result, the historian interested in this sociology is faced with “an enormous corpus of knowledge formed over more than a half-century by this “Muslim sociology”¹ and which seems to be unusual in more than in more than one respect” (Roussillon 2002:193-221).

Once the protectorate was established, the role of the scientific mission and the “sociologists” of the general residency² remains the same, but the objectives more specific, which we will limit ourselves to extracting from the work of two eminent sociologists of the residency: Michaux-Bellaire and R. Montagne:³ the main concern of the former was to show that the total Islamization of Morocco was a false idea that must then be rejected: As we more deeply penetrate the Moroccan system”, he says, “we are able to realize – through the veil which covers it with a uniformly Islamic appearance – that a large number of institutions which make up this system originated before the Islamization of the country (Michaux-Bellaire 1927).⁴

The survival of some pre-Islamic customs and traditions not in accordance with Muslim orthodoxy in some regions of Morocco is quite evident, but the conclusions that sociologists during the residency drew from them are false. Customs and rites not in accordance with the *sha'ria* (Islamic law) existed and still exist in Morocco in various forms, without, however, stripping the practitioners of these mores their faith in Islam⁵ or their Islamic identity. In this framework, the attitude of defense of the “Azerf” (traditional Berber law) adopted by the authorities of the protectorate was also an integral part of the segregationist aim of the Berber policy conducted by these same authorities, because:

Beginning in 1914, the promise had been made to the dissident populations that they would be governed by traditional law applied by “djemaâs.”⁶ A census was taken of the Berbers to learn which tribes followed tradition and which the *sha'ria*. The assemblies worked to the general satisfaction. A commission met, from which came the famous “Dahir” (law) of 16 May 1930, thereafter called the “Berber law” (Coatalen 1970).

This Berber policy of colonization was definitely based on the knowledge compiled on the “cities and tribes of Morocco” and “it was Michaux-Bellaire who was the first to so clearly articulate the double opposition on which was constructed both the compilation of colonial knowledge and the “Muslim policy” of the protectorate:

The Arab/Berber opposition that intersects that between mountains and plain and nomads and sedentary groups which continues in the representation of an Islamity which was said to have been imposed on the

indigenous populations and their “residual paganism” behind which this sociology was close to seeing a “secularism,” or even a “republicanism” – to the point of attempting, with the promulgation of the “Berber Dahir” in 1930, to bring these supposed divisions into play to consolidate the colonial stranglehold” (Roussillon 2002:193-221).

This “*Dahir*” was the result of the Berber policy of the general residency which made the *Azerf* official with the objective of dividing the Moroccan society into two fractions: one the pagan Berber governed by tradition and not by the sha’ria (Islamic law), forming a linguistic group hostile to Arabization, living in chronic political anarchy (*Siba*) (anarchy) because of their refusal and opposition to central power (*Makhzen*); and the other composed of Muslim Arabs, subjected to “Makhzenian” authority. This opposition between *Bled Makhzen* and *Bled Siba* “appeared to observers including Michaux-Bellaire, as the “formula” which presided over the operation of the Moroccan political system that the Protectorate should be interested in preserving” (Roussillon 2002:198) in order to successfully conduct its policy of local administration.

Thus, it appeared that the defense of the Berber originality was adopted only to serve as an arm against Islam, against the Arabic language and Islamic law:

“We must avoid Islamizing or Arabizing the Berbers; if it is necessary for them to evolve, we will direct their evolution towards a clearly European culture and not purely Muslim,” was stated in the 1914 report (Coatelen 1970:6).

To speak of total pseudo-Islamization of Morocco and to cast into doubt its religious unity was aimed at rallying a part of the population to Christian civilization. At least, this is the conclusion that young Moroccan nationalism drew from the clauses of the “Berber Dahir,” and this is what comes out in Michaux-Bellaire’s remarks when he states that:

The Muslim period is one of the periods in the history of Morocco, let us even say its principle period, but it is not all of its history (...) I am certain that this clarification will allow us to locate, through often deceptive appearances of official and classical Morocco, the true body or more precisely the diverse bodies of which, in reality, the Moroccan State is composed, and which it is in our great interest to be familiar with” (Michaux-Bellaire 1927:1-25).

The objectives targeted by the sociology of Michaux-Bellaire coincide with those of Robert Montagne’s sociology through what P. Coatelen qualifies as false evolutionism which “consists of attributing to a foreign culture to the western observer a period of history in the West” (Coatelen 1970:8).

Thus, M. Bellaire tries to show, through a “religious archeology” that Morocco has gone through several pagan, roman, Islamic periods, and why not a new European (Christian) period as the evolutionist laws claim?

The evolutionist approach of R. Montagne proceeds, according to the terms of A. Laroui, with a “political archeology,” faithful to the same objectives. And it is from Masqueray that Montagne borrowed the idea that the Berbers are in the social state of the Greeks before Athens or of the Latins before Rome: “In our view, the inhabitants of Souss,” he says, “should maintain up until the present, in their remote valleys, the obscure and agitated life that the people of the Mediterranean knew before the development of the cities and empires of Greece and Rome” (Montagne 1930:53).

It was thus in the Berber mountains in the South of Morocco where Montagne went to look for what A. Laroui calls “the institutional tribe of classical history, the tribe that Rome destroyed in Europe ” (Laroui 1977:168). This tribe maintains its “purity” thanks to its independence and its refusal of central power (Makhzen), thereby forming “republics” living in the “Siba.” This “Siba,” which is to become, according to A. Laroui, “an institution: it was the delight of anarchy; the less the tribe is “makhzenized,” the more quickly the institutions of the past can regroup” (Laroui 1977:70).

R. Montagne, in the foreword to his thesis on the Berbers and the Makhzen, elucidated his objectives which are the same as those of Michaux-Bellaire:

Going through often rapid transitions, prescriptions of particularly primitive customary law – although very much alive and sometimes marvelously adapted to the economic circumstances of existence – to the rigid rules established by the holy legislation of the Qu’ran, these Berbers see, after their submission, the quick ruin of traditions to which they were secretly the most attached” (Montagne 1930:XI).

The protectorate’s Berber policy, with which the sociologists of indigenous affairs were associated, was crowned by the promulgation of the Berber “*Dahir*” which distinguished Berber regions from Arab regions ; the former were governed by tradition (Ürff or Azerf), the latter by Islamic law (*Chraâ*). As P. Coatalen accurately notes, this “*Dahir*” provided new Moroccan nationalism the chance that it was waiting for, and “the fiasco of the Berber policy illustrated the falseness of the theses on the Berbers” (Coatalen 1970:8).

Colonial sociology had as its motto “Divide to conquer,” but after all, as Edmond Burke says: “It is hardly surprising to note that colonial sociology was colonialist, what else could it have been? (Burke III 1979:38).

It would therefore seem useless to restate here the inventory of criticism of which sociology has been the subject, because our goal was to define the objective, which can be summarized as follows: attempt to update and restate the underlying or apparent antagonisms which govern or seem to govern the Moroccan organism. A sociology of national obedience should, in our view, serve opposite ends.

Sociology post-independence

Up until the independence of Morocco (1956), Moroccan sociology was the work of foreigners: "Without questioning either their good faith or their value, we can still lodge the criticism that they had only an extrinsic knowledge of their subject," said André Adam who adds: "Such is without fail sociology in Morocco, the work of the French, Spanish, English, Germans, and Americans. The time for this paradox is over" (Adam 1972:41).

But is this paradox really outdated? Can we argue that fifty years after the political independence of the country, the study of the Moroccan society has become the work of Moroccans?

We doubt it, although the number of Moroccan sociologists continues to grow and they continue to conduct research on various phenomena and regions of the country.⁸ In fact, we note that at the same time, an increasingly larger number of foreign sociologists continue to be interested in Morocco, and they benefit from means and opportunities not always at the disposal of the national researcher. It is the English-language researchers especially who have appeared in ample numbers since the 1960s and who continue to produce an increasingly abundant sociological literature, more and more advanced, theoretically speaking, and dealing mainly with political, ethnic and religious fields. In our opinion, this sociology is continuing and enriching the projects and objectives conceived by pre-independence French-language sociology, without, of course, expressing its goals and objectives as formally and clearly as the French-language sociology of the colonial period.

Without going so far as to accuse this new foreign sociology of being neo-colonialist, we question its secret objectives, and this is what is driving the new generation of Moroccan sociologists to mention the need for epistemological vigilance, which should lead to a decolonization of Arab and Moroccan sociology which can only be accomplished, according to Abdelkébir Khatibi, from the standpoint of a double critique:

- a A deconstruction of logocentrism and ethnocentrism, this word of self-sufficiency, par excellence, that the West in the process of developing, has developed on the world. And we have much to ponder from this side about the structural solidarity which links imperialism, in all of its iterations (political, military, cultural) to the expansion of what is called social science. (...)
- b This also assumes, and requires just as much a critique of knowledge and discourse developed by the various societies of the Arab world about themselves" (Khatibi 1983:48-49).

"We are thus targeting," adds Khatibi in another article, "a double coordinated movement only capable in our opinion of surpassing simple reproduction and

opening up to sociologists the possibility of a less alienated, practical knowledge, more adapted to the specificity of the subject analyzed" (Khatibi 1975:1).

This double critique initiated by Khatibi (1983) continues to rally several other Moroccan sociologists, including the late P. Pascon and Abdellah Hammoudi, among others. But we believe that this double critique should neither absorb all of the efforts of Moroccan researchers nor make them forget their main task, namely the pursuit of the study of their own society. This task is linked to the birth of Moroccan sociology, and to the definition of its theoretical subject. Therefore, we believe that after having revealed the objectives and the "divisionist" and "segmentarist" underside of colonial sociology, we should in its place substitute a national sociology with a more scientific purpose, guided and mobilized by the concern with emphasizing the unifying elements of Moroccan society,⁹ which make for a one and single nation, and that the colonial sociologist tried to hide, neglect and push aside because he recognized the dangers that these elements presented with respect to his plans.

We cannot currently define Moroccan sociology by a specific subject which is peculiar to it, therefore we will settle for defining it by certain aspects of its objectives. From this perspective, we can argue that Moroccan sociology should have the objective of consolidating the unity of the nation state. This objective will remain nothing but wishful thinking if we do not translate words into action. The ball is now in the court of Moroccan sociologists who, according to A. Adam, "benefit by right of birth from the privilege of knowing their own society from the inside, but who must acquire the difficult art of detachment" (Adam 1972:42). "They possess assets that foreigners did not have. They should remember to watch out for what they think they know. "Science," said Gurvitch, "is the knowledge of the hidden" (Adam 1972:72).

"Detachment", "Objectification" and "Commitment"

"Insofar as it is reflection about society, sociology implies detachment, i.e. the realization of this minimum of rupture with respect to oneself and one's own group, which is necessary for objectification" (Pascon 1986:61).

This detachment necessary for objectification is thus a distance in relation to the subject. Classical anthropology provided this distance to the researcher to the extent that he or she had to travel to another place to search for the subject, which allowed him or her to acquire the sense of comparison, relativity, and thus a certain degree of neutrality:

"By developing itself through the study of small societies foreign to ours and geographically distant, anthropology, to a large extent, saved the cost of the necessary divestiture, and freed itself from the subjective aspects of philosophical anthropology" (Breteau & Zagnoli 1983:8).

It is this detachment in relation to the subject which allowed for the birth of the science: "In fact, generally, sciences were developed from what brought us the least into question, by freeing themselves from the projective identification which centered the environment on man, and led him to see it in his own image" (Breteau & Zagnoli 1983:8).

But spatial distance is not all, and cannot always free us from certain prejudices and preconceived notions:

In fact, "There was a time, which is not behind us yet, when anthropology professors asked researchers to 'have no idea' before beginning the study of a society, as if the mind could become the blank slate that it never was" (Cresswell & Godelier 1976:8).

And it is for this reason that Breteau and Zagnoli note that in addition to distance, a 'decentering' is necessary, i.e. of guaranteeing a "distance both with respect to the other and to oneself" (Breteau & Zagnoli 1983:8). And all with the goal of this "objectivity" which is the condition *sine qua non* of 'scientificity':

"Classical science is founded on objectivity, i.e., a universe made up of isolated objects (in a neutral space), subjected to objectively universal laws" (Morin 1977:96).

It is this idea of objectivity which was the source of the birth of positivist sociology: Saint Simon and A. Comte took as their objective the creation of a social science whose archetype always depends on physics: thus the creation of social physics which is a science like any other, if not the "supreme science":

A. Comte said, "I mean by social physics the science which has as its particular subject the study of social phenomena considered in the same way as astronomical, physical, chemical and physiological phenomena, i.e., as subject to invariable natural laws, the discovery of which is the special objective of its research" (Comte 1972:86).

Durkheim, whose sociology remained faithful to the same positivist tradition, had the same inspirations and aspirations, in considering that social facts: "consist of ways of acting, thinking and feeling, external to the individual, and which are endowed with a power of coercion by virtue of which they make their presence felt to him" (Durkheim 1977:5).

The exteriority of social facts in relation to individuals goes hand in hand with the first and the most fundamental of rules of the sociological method, according to which "social facts should be considered as things." In both cases, the goal is to create a certain distance between the sociologist and his subject. This distance, adds Durkheim in the preface of the 2nd edition of the "Rules," can only be "mental attitude," because the principle of "conscious negligence" is hardly respectable in social science.

Thus, the eternal problem remains open: should science cast off ideologies that reflect the consciousness of a group or class? With respect to this question, the response of the Moderns is that:

the sociologist has a responsibility with respect to the society which he is examining (whether he lives in that society or is visiting it) and this responsibility binds him both vis-à-vis the investigation and the explanation that he proposes (Rivière 1969:20).

Because, according to M. Grawitz:

the absence of objectivity implies a number of nuances, from erroneous description, bias, down to the simple fact of the preference for such and such a field, and the use of such and such a technique, the degree of necessary objectivity varies according to the field and the type of observation in question. We must carefully distinguish between the description of facts, which should always be objective, and the interpretation which can be more personal" (Grawitz 1976:321).

True objectivity is not then adopting the most neutral posture possible, but "consists of recognizing one's own commitments and personal biases which may result."

For comprehensive sociology, objectivity does not lie in a pseudo-detachment, the search for which could, on the contrary, be an obstacle to the study of social action, a study which should be as intrinsic (subjective) as extrinsic (objective), and be so without questioning the 'scientificity' of the mode of sociological knowledge. Objectivity in sociology then becomes, in the last analysis, a question of ethics (or moral responsibility), or axiological neutrality which comes under a certain ethics of the profession of sociology rather than *a priori* respect for certain given methodological precepts. It is the very essence and the primary objective of all sociology of a national and nationalist persuasion, and an academic and activist persuasion at the same time. This being the case, and all the while indicating the persistence of this line of thought, we can advance the diagnostic that current Moroccan sociological literature shows that this nationalistic "commitment" is losing followers, given that sociologists of the new generation, following the example of their peers in the Arab world, are adopting the most varied postures and positions.¹⁰

Notes

1. Alain Roussillon has stressed the paradox of the term "Muslim sociology," adopted by the precursors of this sociology and which took the form of an "accumulation of knowledge for others" (Roussillon 2002).
2. Rare among these were those who were sociologists by training. The majority were military leaders or civilian comptrollers who converted into sociologists at the request of colonial officials.
3. We limit ourselves to the example of these two sociologists because they were more involved along with the colonial officials. The work of certain other sociologists (J. Berque and Ch. Leceour, for example) was more concerned with truth and clearly distinguished itself from colonialist sociology.

4. Cited by Georges Nicolas (Nicolas 1961:187).
5. Paul Pascon deals with some customs and beliefs not in accordance with Islam, and still existing in Morocco. See his article: "Myths and Beliefs in Morocco," in: *70 Years of Sociology in Morocco*, B.E.S.M. n° 155/156 (1986). In another article entitled "Anthropology and Colonialism," Pascon notes that Islam as an ideology of the State and citizen was ferociously attacked by the colonialists following the example of Edmond Doutté, for whom "the more primitive a people is, the more religion invades all of its institutions; I do not believe that this principle is seriously contested today; it is one of the best established principles in sociology." Pascon comments on this principle, saying "except that still today, we do not know what primitivism is. As for religion, we have a tendency to extend the meaning to it. Capitalist Ideology discredits religious forces and accuses them of Barbary when they stand in the way of colonial barbarism" (Pascon 1982:253).
6. *Djemaâ*: tribal representative assembly in Morocco.
7. In his article op. cit. A. Roussillon, basing his remarks on A. Laroui, gives the following definition of the idea of "Makhzen" by saying: "Lit.: store, warehouse. More than the State itself or the administration, this term for the stranglehold of central power on society, including representatives of the sultan to the different levels on which his authority is exercised."
8. This research is often the subject of university theses. Moroccan sociological production remains weak with the exception of these theses, which can be explained not by any lack of nationalistic fervor, but by the absence of well-defined research projects which respond to any scientific or social demand.
9. In our thesis on "Rural Exodus in Morocco," we attempted to highlight the contribution of a sociology of current migrations in Morocco to such a national sociology.
10. Ali El-Kenz distinguishes the three following categories within the community of current Arab sociologists: the academic, the activist and the consultant: "These three figures, he says of the researcher in social sciences, can be found today in all Arab countries through the most varied combinations, depending on the disciplines and the countries. We can even sometimes observe with the same researcher, a mix of different postures – academic and activist, academic and consultant, or even activist academic and consultant, depending on the most varied proportions" (El-Kenz 2004).

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8

Autochthones Making their Realities Strange in Order to Better Understand Them

Roseline M. Achieng'

In my paper titled “African(ized?) Gnosis as Sense-making?”, (Achieng’ 2005:54-57) one cannot help noticing my heavy borrowing from already existing debates on methods in the social sciences. One can ‘justly’ claim that indeed, there is nothing new the paper proposes! Undoubtedly, in as much as we researching African environments would like to formulate our own methods for doing research, I argue that it would be advantageous for us to first epistemologically¹ reorient our standpoints in order to discover our own methodologies² and consequently methods³ of doing research. I further posit that we are not operating in a social science vacuum. Rather, a lot in terms of knowledge production, transfer of knowledge and exchange has occurred, such that for researchers in African environments, the ‘catch 22’ is to find our place in the universal wheel of knowledge and contribute to its recognition and sustenance. It would, therefore, be justifiable to claim that whereas we could operate with the already existing research methods, it would be of profound importance to discover our own epistemological and, consequently, methodological standpoints. To this end, this chapter is a contribution towards the discovery of a methodological standpoint for African environments for those subscribing to a particular epistemic community⁴.

I argue from the premise that each society has a way in which it organises its knowledge and passes this down across generations. Varying from context⁵ to context, this knowledge is handed down through oral or written modes of communication, or through material culture (mostly artistic works). This knowledge shapes how those who belong to a given society comprehend, explain and view occurrences within their own world or those of others. Hence, all of us have a way in which we approach and understand things. Conclusively, one can claim that how we approach things thus depends on the context from which we are discussing or the context which we have acquired.

The knowledge we produce that flows from a particular context is organized in a particular way. Phenomenologists refer to such an organization of knowledge following certain categories as typifications. We come to know about this organized knowledge through what is handed down to us as knowledge across time, through lived experience and encounters, and through observation. This knowledge is presented to us through the process of socialization. We eventually internalize this knowledge in the process of our interaction with others sharing the same context. In the process of socialization where knowledge is presented to us one has to be acquainted with the forms of expression emergent of a context as a medium of grasping this knowledge.

In the very process of internalization, such knowledge becomes commonsensical, taken for granted or natural. This is for the reason that one partakes of this knowledge every day. Many things are done in a routine manner even unconsciously without asking every time why something is being done in the way it is. This knowledge is so naturalized that it becomes difficult for one to see anything 'strange' or 'new' in it.

Now, if we take the latter as our point of departure, the question that comes to the fore is, given that African researchers more or less research their 'own' naturalized contexts, how do they question this commonsense knowledge in order to understand it in new ways? Put differently, how do autochthones as partakers of the naturalized knowledge in their contexts begin to question this very knowledge? How do autochthones begin to see their everyday realities in a new way in order to explain it differently from the taken for granted way? Conceptually, the question at large is how does a subject research itself without being subjective and thus biased?

It has been variously argued that autochthones cannot research 'objectively' their own contexts. At best, they can only regurgitate what is handed down to them as knowledge without raising critical questions about it. For, as the claim goes, they cannot 'see' in new ways and thus conceptualize on the reality that they partake of!⁶ Thus for African social researchers, the big question is how do I make my everyday reality 'strange' in order to better understand it? Conceptually, the question is how do autochthones strike a balance between being subjective (knower) and obtaining objectivity (take a distance to what they know in order to explain it in a new way). Certainly, the question of what is true objectivity is one that is still strewn with philosophical debate (see some of the chapters in this volume that have addressed the question of what true objectivity is). My working definition of objectivity is to understand the true situation as it reveals itself. But what is true and whose truth counts? Truth to me is what is agreed upon by actors in a context as what holds true for that situation, such that truth is relative and can change according to context. Therefore, there are many truths and this depends on the context we are in. But how does this truth reveal itself to us as autochthones?

Three Theses in Support of Sensemaking as a Methodological Standpoint

As autochthones, we are both the subjects and the objects of research. We have a double effect – like two sides of the same coin. This is what I call a mirror effect. Consequently, we have to make sense of what we see, experience or partake of. Sense making as a possibility of a methodological standpoint for autochthones is based on two foundations: getting subjective and maintaining objectivity and representing social reality as it really is by discovering the hidden meaning behind the reality that is unfolding itself to us (going to the truth itself).

But how do we take a distance to the reality we partake of in order to better understand it and in a new way or go to the hidden truth?

Trans-historical methodology

This is through analyzing historical periods with an aim of tracing the changes in the social order. The objective will be to discern which structures there were, what type of action brought a change in the structures and why there was a change in the prevailing order. These, however, should not be taken in isolation but inter-linkages analyzed in order to account for continuities, discontinuities or new modes of doing. Furthermore, this trans-historical methodology should not be understood as going beyond the threshold of history. Rather, we confine ourselves to the past and present activities in order to understand the 'there and then' and make sense of the 'here and now'. This means that we have to go deep into the context and excavate all background information. Rich background information of the changes across historical periodicity helps us in beginning to see things in a different way and thus begin to question why something is like it is now and not like it was before.

A comparative methodology

As we have seen, living in different contexts presupposes seeing things in different ways as those living within that context. Though we are autochthones, we have different contexts, which we can oscillate in order to make the taken for granted strange. We have rural-urban environments with different kinds of neighbourhoods. The latter harbour people experiencing diverse lifestyles, living in different conditions and thus bearing knowledge of divergent realities depending on the context one is in. We have different climatic regions, from desert to semi desert, equatorial rain forests to swamps and grass-lands presupposing different ways of doing and diverse experiences. We are divided into Northern, Southern, Eastern, Western and Central regions. We interact with people from different hemispheres. Comparison of one set of circumstances to another could be a strategy for autochthones to engage in, in order to better understand their reality.

Comparison can be done in three ways:

- (a) Contextual comparison: This is through a deep contextualization process of the social, political and economic conditions and transformations over time in different contexts. The aim could be to look at the similarities and dissimilarities, continuations and discontinuations in order to understand and account for (by giving explanations to the why and how of processes and the changing dynamics).
- (b) Methods: A triangulation of methods in a comparative way could also assist autochthones to make their realities strange in order to better understand them. The question to ask here is why one method produces a certain set of information and the other another sort of information.
- (c) Different categories in society: Through engaging perspectives from different clusters of people in an intergenerational manner, people from different regions, gender, ethnicity and racial dispositions, autochthones can make their realities strange. This is for the reason that the different categories in society will have different explanations to the same observable reality depending on their lived or shared experience. Therefore, the task of the autochthon researcher would be to make sense of these different explanations by trying to find out the hidden meaning in explanations derived from the different categories of people in society.

Multidisciplinarity

The question of engaging in a multidisciplinary methodological approach is indeed a difficult one. The critical issue is how scientists with different ways of looking discuss? How can perspectives be integrated? How can one avoid paradigmatic and conceptual quarrels as different ways of seeing or viewing reality are introduced? In my opinion, engaging in a multidisciplinary dialogue can only proceed at a conceptual level or at the level of generalizations. This is for the reason that multidisciplinarity necessarily involves different methodologies or ways of looking at the same reality. To reconcile these divergent ways into one particular way of viewing reality would be succumbing to the development of dogmatism. I am of the view that, methodologically, multidisciplinarity as a standpoint can successfully be adopted at the level of conceptual analysis. A typical example to illustrate this is research on HIV-AIDS which could easily involve political scientists, social scientists, philosophers, medics, and those in the medical, chemical and physical sciences. In such a manner, different ways of viewing the same reality are introduced. However,

how each scientific discipline is to proceed in viewing the reality is a disciplinary issue that cannot be resolved in a matter of fact way. As an example, how can political scientists basically interested in macro structures and their functions reconcile their methodological standpoint to that of chemical or physical scientists interested in micro organisms? Engaging in the particulars of a discipline would thus not move any research agenda forward. A multidisciplinary approach would at best dwell on the generalizations (conceptualizations). The task of the researchers engaged in a multidisciplinary standpoint would then be to account for the why and how of the different explanations, with the aim of not only uncovering the underlying truths, but also seeing the reality in many different new ways; and in this way develop further questions into the why of occurrences.

A Note on Methods

If we adopt sense making as one of the basis of a methodological approach for people circumscribing to a certain epistemic community, then we will be necessitated to also interrogate our methods of inquiry. For those who are familiar with the history of qualitative research, we know that ethnographic methods, mainly of the Chicago and Manchester schools in the 20s, were involved with the notion of the other. In other words, ethnography grew out of the interest of knowing how the other (named primitive people) lived. The methods here were mainly participant observation and the narrative interview method.

Whereas I agree that for African realities, because of the oral tradition that still characterizes our medium of communication, our version of the narrative interviews or *badithi* is still a valid way of collecting information, I query the credibility of participant observation as a method of collecting information. Participant observation presupposes that an ethnographer leaves her/his community to go and stay and observe another community that is not one's, own. However, as autochthones, we are already participants studying our own realities. The paradox is how we who are already participants in a culture also participate in observation?

I propose a move from participant observation to communicative observation for autochthones. We thus need to be communicative observers. By communicative observation, I mean engaging in critical observation and critical questioning (elements of which I have explored above). Communicative observation also means that we become aware of other means of expression in the community such as artistic works and material cultures; for example, song (music), masks, carvings and billboards.

Notes

1. By epistemology, I mean how we come to know.
2. By methodology, I mean how we approach what we want to know.
3. By methods, I mean the tools we use in searching for what we want to know.
4. By epistemic community, I mean those subscribing to a particular way of knowing and a particular approach in doing research.
5. Apart from the territorial, in my view, context also implies shared experience.
6. El Kenz (2005) has explored it in terms of the quarrel between 'anthropos' and 'Humanitas' and the struggle that ensues when the former wants to become the latter.
7. See one of the standard books on participant observation by W. Foot Whyte (1943) *Street Corner Society* where the author engaged in qualitative research with a gang of street-boys in an Italian town.

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9

Life History and the Writing of Ethnography: The Case of Morocco*

Mokhtar El Harras

In this chapter, I will try to raise some questions about the complexity of collecting life histories and presenting them in ethnographic writing. I will try to show that the way in which life histories are obtained has a great impact upon the way they should be presented. I will formulate my analysis in the light of some Moroccan examples.

The Complexity of the Life History

No anthropologist can deny the complexity of life history. One of the main problems we face while studying this method is that we do not know exactly who the speaker is. Can we say that the narrator is the only true speaker in the life account? And when he says I or me, is he really talking about himself? The difficulties in giving an adequate answer to this question have led some analysts to separate the narrator from his discourse. The life account contains more than one voice and more than one narrator: the life history is thus a result of conflicting and antagonistic levels in the narrator's own mind. In other words, there are many selves in the life account: there is the 'I' character (the narrator as object of his own account), and the 'I' narrator (the narrator as observer of his life), each formulation permitting the narrator to give us an image about himself without necessarily using the pronoun 'I' as judgement or acknowledgement of the opinion of others (Chabrol 1983:81-82).

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The narrator speaks sometimes to himself using 'you', particularly when he evokes some ideal he believes he should fulfil in his life. He sometimes uses 'he' when he wants to reveal the image the group has of him, or 'we' to attest to his integration into the social group. Even though the narrator says 'I', it is probable that he refers only to the image he thinks is adequate to portray him. Because of the selective characteristics of memory, the narrator is not necessarily the most competent one in knowing himself. Moreover, between the image that the group has constituted about him and the true group image about him, we can often find both inaccuracy and opposition (Poirié, Clappier-Valladon, Raybaut 1983:56-57). The life account does not come from a coherent source of discourse, and every attempt to relate it to the same origin cannot be but an illusory practice. If the life history seems in its final version to be an adequate representation of the narrator and his discourse, it is because the researcher, as interlocutor, analyst and interpreter, refers all the voices back to the narrator, thus transforming a true diversity into an illusory unity (Chabrol 1983:82).

Other complexities are connected to the interview situation. It is known that the interviewee may sometimes tell what the researcher would like to hear, and at the same time, might limit the data to what interest the informant has or to what might enhance his image after death. In the life history interview, there is thus a third character who intervenes between the researcher and the narrator: death. Its presence converts the life account into a defence against death and an attempt to keep some kind of existence in the afterlife (Levet-Gautrat 1983:119). In the same way, the life account sometimes gives priority, not to the information needed, but to the task of giving life lessons and experiences presented to the researcher as ideals he should fulfil in his life.

It is also very probable that the interviewee will give a different version of his life if he interacts with different fieldworkers. Much depends on the nature of the interaction between the two personalities. It is possible that he speaks about an ideal personality instead of a real personality, just as he might insert his account into the mould of mass media. Furthermore, we know that human consciousness is not able to be totally aware and perceptive of the present life. Often we understand neither the logic which has an impact upon our present actions, nor the significance of contemporary events. Our consciousness lags behind our actions, and our memory seems unable to catch up with what was well understood at the time it happened (Veyne 1971:229-231).

Linguistic mediation is another obstacle between the narrator and the researcher. The narrator might leave out some personal or family events only because he does not have the proper words to discuss them. Or he might exaggerate these events because of his sophisticated language. In both cases, memory does not have easy access to its object. Moreover, because the significance a narrator gives to events is a result of their interpretation at the time they happened, as well as at

the moment of their memorization, and because this memorization is first of all determined by the present situation of the narrator, the significations expressed by the narrator will necessarily be subjective and susceptible to change (Bertaux-Wiame 1985:50-51).

For example, in the rural area I have studied in North-western Morocco, I have noticed the following obstacles to the life history method.

- 1) Spatial and social mobility in this region influences greatly the effort of memorization (Amphoux, Ducret 1985:198). It is well known that there is an intense emigration from the countryside to the cities, as well as change of social status from low to high, and vice versa. To ask a townsman to recall the half of his life that he spent in the countryside often seems to be a difficult task, for his memory has lost its spatial reference mark. In the same way, to ask a rich or well-off peasant to recall a part of his life when he was a sharecropper does not seem easy, because here too the memory has lost its social reference point.
- 2) The informant only narrates the facts that were considered relevant from his point of view. He is selective in his life account. A peasant may speak particularly about his work 'life'; a former *caïd* may speak particularly about his political and administrative life; while a *sherif* (The Prophet's descendant) may show above all his propensity to reveal his family genealogy. The memory eliminated – consciously or unconsciously – all that seems to the narrator contradictory to the image he may want to present of himself.

While women work hard in this society – going to the market almost every week and carrying heavy burdens for long distances – the discourse of male narrators usually neglected these facts and showed women's roles only in social exchange and reproduction.

Moreover, while a woman participates actively in decision making, the discourse of the male marginalized not only her participation in family matters, but also her presence as a person. In the same way, when we asked men to give us their family genealogies we noticed that their memory became weaker as they began to talk about women, but stronger with regard to males, particularly when the narrator had many sons. This selection comes perhaps from the reluctance of this population to talk about their wives with an outsider, or maybe from their fear of losing their maleness and virility in face of the researcher. It could also be a result of a vengeance operated by a discourse against the real and the empirical. What is certain, however, is that this selective discourse is somehow a result of what might be called 'structural oblivion'. It means that the male, because of his dominant social position, is unable to give the female all the importance her real presence deserves. He forgets women not because he is a male, but because of his dominant social and economic position. Furthermore, it means that the image the

narrator seeks to present of himself is determined not only by what is being remembered, but also by what is being forgotten. Maybe oblivion is more important in this matter than memory (Mauve 1985:26).

I also noticed that a life history sometimes becomes a kind of peasant defence toward a researcher as a townsman. The peasant seeks in this manner to modify the image that townsman have constituted about rural dwellers. It is known that family education in traditional cities has for a long time tended to depreciate the image of the people of the countryside. Before the colonization of Morocco, tribesmen were able to defend themselves by attacking the cities. This is no longer possible. Therefore, the only weapon that remains in their hands is to modify their image in the townsman's mind. Some sentences that were collected are very significant in this regard: 'Don't think that the people of the mountains are silly or ignorant or that because they don't live in the cities, they don't know what happens in the country'.

Sometimes, the peasant borrows ideas and conceptions from the cities to show you that there is no difference between the researcher and him, between the city and the countryside. Consequently, the researcher finds in the narrator's discourse nothing more than his own image as a townsman.

The narrator might sometimes avoid talking about his participation in political institutions that have since been condemned (e.g. participation in colonialist administration). On the other hand, it is often true that narrators try to seduce the ethnographer by talking in detail about their participation in the resistance against the colonial system.

Concerning the present, the majority of the interviewees make a conscious calculation of what can be said and what cannot be said. As a result of these restrictions the interviewees seek sometimes to be understood without striving to speak clearly, or without continuing their narrative to the end.

With respect to the form of the oral life account, we can notice that, with some interlocutors, it takes the form of written biographies about well-known religious figures (Von Grunebaum 1962:291-306). This is particularly true about local men of learning who have passed through different stages of traditional education. Because they have read many Islamic biographies, they tend, when asked to give an oral account of their lives, to keep the same written form, to the extent that their life history reveals the repetitive and the universal more than the individual and the specific.

Writing Life Histories

For a long time writing has been reduced to method: keeping good field notes, making accurate maps, presenting statistics, and writing up results. The fact that this process has not until recently been challenged reflects the fact that many researchers still believe in the possibility of having immediate contact with experience, the direct perception of an underlying reality. This ideology seems,

however, to blind the anthropologist to the fact that the 'translation' of indigenous culture, wherever it happens, 'takes place within relations of weak' and 'strong languages that govern the international flow of knowledge' (Clifford 1986:22), and at the same time, implies a shift from non-literate and non-academic culture to written and academic language, with the implicit claim to superiority of the anthropologist who presents himself as an outsider who knows the inside life of the population he studies, and gives to it, through a text, the possibility of persistence (Asad quoted by Scholte 1987:42).

Many life histories are written on the basis of a salvage allegory, of saving in the text what remains of vanishing traditional society and culture. It is in this way that ethnography legitimises its writing practice as inscription of a culture rather than transcription, as representation rather than evocation. And behind this salvaging and redemptive action, there is the idea of the other society as being weak, and thus in need of being represented by an outsider. There is also the idea of the ethnographer as a custodian of an essence, the privileged witness to an authenticity that cannot be easily refuted (Clifford 1986:112-113). Because the culture he studies may vanish, and because the ethnographer tries to convince us of the truth of what he says by presenting factual observations in a theoretical context, we are inclined to believe him simply because he has 'been there'. He does this in such a way that every one else who would decide to return to the fieldwork in order to check the seriousness of the ethnographer's statements, even if they find different facts and use different models, would not be able to conclude anything more than that things have evolved and changed from their previous state (Geertz 1988:4-6).

Such representations of anthropological knowledge have now begun to change. Anthropology, which was born in colonial conditions, is beginning to readapt itself to new processes of decolonization. The widespread use of the media and the role of tourism now deprive anthropology of such assumptions as the separability of the subjects of study and the audience. The people who ethnographers study are no longer colonial subjects, nor mere objects. Their increasing capability to have their own view about their culture is now making the anthropological encounter one of dialectical interaction (Geertz 1988:131-135).

It is not surprising then to find that, in many cases, the life account combines oral expression with the reading of personal and familial texts. I have had the experience of narrators who interrupt the interview to bring me written documents concerning matters such as marriage, property ownership, inheritance and legal cases. The ethnographic work is no longer a mere written record of an oral account. As noted by James Clifford, data move from text to text, and both informant and anthropologist are readers and re-writers of the life history (Clifford 1986:116). The researcher is no longer the primary bringer of the culture into writing.

Furthermore, what a narrator communicates to the researcher is not only an oral account but is also an oral text. What distinguishes the life history method is that objectivation exists not only between the researcher and his subject matter, but also between the narrator and his life. The narrator has a kind of theoretical relation with his life. Instead of just reproducing it, he submits it to selection, reorganization and reinterpretation. The oral life account is thus itself a writing (Kishani 1985:71-72).

If we then observe the passage from 'real' life to the oral text, and finally to the written text, we can imagine how the loss is important (Zonabend 1985:36). That is why J.J. Rousseau privileged the direct observation of the world and viewed the book as contrary to truth and science. That was also the reason that incited Jacques Derrida to conceive writing as violence perpetrated against oral accounts, and also oblivion (because all is written, the memory is no longer needed). In the same way, Claude Lévi-Strauss viewed writing as a means by which human societies lose the immediacy, the face-to-face communication and the intimacy of speech (Derrida 1967:55, 198).

The life history cannot be obtained without cooperation between the researcher and the narrator; the relationship between them cannot be viewed in accordance with the observer-observed dichotomy. For, instead of being just an observer or object of observation, both cooperate dialogically to produce a discourse (Tyler 1986:126). The life history method deprives the researcher of the epistemological privilege that has been given to him by the structuralist tradition. Consequently, if the narrator's life is neither an object nor a series of facts, we cannot deal with it simply by procedures such as 'descriptions, inductions, generalizations, verification, experiment, truth'; the mode of ethnographic writing must be evocation rather than representation, a version of the life history rather than the true life history (Tyler 1986:130).

But although cooperation fails in many cases, we find that only a few writings about Moroccan society show in some detail the difficulties the anthropologist encounters while doing his fieldwork. What we notice instead is the image of a researcher in good terms with the people whom he has been studying. Is this the reality? I think that both inside and outside researchers encounter features that might encourage them to continue their work, as well as what might discourage them from continuing to do so. We do not find in the text the strained and unstable relationships that might sometimes emerge between the researcher and some of the individuals he studies. This means that agreement and mutual sympathy do not always proceed from the fieldwork experience, notwithstanding the image some anthropologists like to project of a researcher who dominates either the techniques he employs, or the ability to get on well with the people he is studying (Clifford 1985:61).

Moreover, in spite of the curiosity aroused by the presence of the author in the field, where he usually becomes a focus of attention within the community, and where he finds himself, if not giving up some views over his own life, at least questioning some of its aspects under the influence of his narrator's life; in spite of the fact that his fieldwork results are largely determined by the way he interacts with the other and intervenes in his subject's lives, we do not notice the presence of the author in the text, not as a writer, but as a fieldwork researcher. The fear of colouring objective facts by his subjective views often leads him to suppress his presence in the text, or at least to limit it to preface and notes (Jarion 1974:626). He acts as if there were a contradiction between objectivity and manifesting explicitly his presence in the text. He writes a text which should be, in his view, related not to an author, but to specific facts or events. And the more facts and events he presents, the more he thinks of his writing as being objective. By acting in this manner, he becomes like the historian who thinks that the credibility of his work depends, above all, on how many documents he can include in his text, on the extent to which his text can become equivalent to actual events, and his presence as author limited to its minimum expression.

We notice also that the life accounts, when collected, become in the anthropologist's laboratory an object of division and disintegration. I mean that the presentation of the life account is repeatedly interrupted by the author who intervenes through it to clarify, to explain or to make some comments. In any case, what is usually meant by scientific work is seen as being the opposite of the novel where such processes as identification, pity and admiration are possible. Have we, however, deontologically speaking, the right to divide a man's life, a man who trusted us and gave to us his life account? Are we here not facing a double game by the anthropologist? Is not the anthropologist's attitude here ambiguous? In the beginning he listens to the life account and records it without any comment. But, then, when he returns to his office, he begins, on the basis of his personal conceptions and values, to disintegrate the narrator's discourse, without giving to the informant the possibility of expressing, if necessary, his disagreement with the author's interpretation. Therefore, the writing might be conceived of, at this level, as a disruptive violence and authoritarian practice quite different from the previous sympathetic listening (Lejeune 1985:81).

This authoritative attitude also appears at the moment of publication. We know that anthropologists sometimes publish accounts of informants' lives that would greatly displease the informants themselves. Anthropologists seldom consult informants about what should be published and what should not.

To limit the authority of ethnographic writing, anthropologists are increasingly adopting dialogic texts in which a plurality of voices replaces the monology of a single author. This only displaces ethnographic authority, because it is still the

author who does the orchestration of all the discourses in the text. The author might modify the narrative order, or suppress some of its parts. The author of a polyphonic text does not suppress his authority, but only modifies its basis. Consequently, to overcome the authorial authority one needs to treat collaborators not only as informants or enunciators, but also as writers (Clifford 1988:43, 44, 51). This evolution is still in process. It needs to rely not on a conventional notion of a shared cultural system, but on a notion of a culture viewed as an outcome of negotiations between subjects through acts of communication, and as incarnating partial truths of each subject's points of view. The anthropologist, instead of trying to impose, in the name of 'being there', one true interpretation of history, must 'encourage readings from diverse perspectives'. And as long as it is the reader, much more than the author, who gives to the ethnographic text its meaning and coherence, the adoption of a dialogic form would not mean a loss of the text unity (Clifford 1988:52-53).

The Case of the Moroccan Society

It is certainly pertinent to give consistency to what has been presented above, to refer to some anthropological studies whose authors have presented their fieldwork in Morocco by using the life history method.

Among these biographical works, we can distinguish between two kinds of writing: the one that uses biography in order to study Moroccan society and history; and the other which resorts to biography as a pretext to evaluate the status of anthropology as well as relationships with foreign cultures.

If the authors, in whose writings the social dimensions of biography were more salient, are admittedly, at the level of their methodological approach, close to each other, they are however interested in different issues raised by their field research. While John Waterbury was essentially attracted by the study of economic behaviour, and Henry Munson by the study of social and cultural change, Dale Eickelman seemed more interested in knowing the nature of Islamic learning¹. Let us see separately how each one of these writings dealt with biography.

In Waterbury's book (Waterbury 1972), we notice that the author gave more attention to what was general in the life of Haj Brahim as a Soussi merchant than to what was unique and intimate in his personality. He aimed, through his biography, to find out the extent to which the norms of thinking and behaviour have changed in Moroccan society. So, instead of letting Haj Brahim speak for himself, he intervened constantly to give a larger sense to his remarks and situate them in the framework of his personal interpretations of Moroccan society and history. He used a great deal of the fieldwork data he collected to inform his treatment of Haj Brahim's biography. Moreover, he approached it in the light of Max Weber's theory about the impact of Protestantism upon economic success, and also from

the standpoint of the hypothesis developed by David Maclelland concerning the cultural and psychological motivations which lag behind economic development. In the same way, Waterbury tried to verify, on the basis of the basis of his informant's life, the extent to which the famous Khaldounian statement about the Maghrebian merchants need for the protection of rulers is still valid (Waterbury 1972: 89-115). And although the author permitted Haj Brahim at times to express directly his opinions and attitudes about Islam, commercial, political and social life, he did not go so far as to give up the role of supplying both continuity and transcendent meaning to such striking events as the sale of poisonous cooking oil in 1959, or to comparable patterns reported in other studies of Islamic and non-Islamic societies.

With the exception of the subject studied and the form of biographical writing, Henri Munson takes a similar approach to social transformations through individual realities. He tries to understand how the status of women, peasants and migrants in the Djeballan highlands of North-western Morocco have been transformed under the impact of diverse factors of change. He also seeks to grasp the cultural basis on which the interviewees built their conception of colonialism, nationalism, dependence and modernization (Munson 1984:3-4). It is within this scope that the author decided to interview forty members of the Si Abdallah family.

His account is not, however, limited just to a number of biographies, but extends to the act and context of narration, and the techniques employed to register it. It is revealed that Fatima Zohra, the author's wife and her uncle, Al-Haj Mohamed, are the source of all the biographies presented in the book. Sometimes, he distributes the narration between them, but in most cases he makes them speak about the same persons and events. The comparison between the two points of view was intended to show him how the narration of Fatima Zohra and Al-Haj Mohamed were, respectively, influenced by western thought and Islamic fundamentalism. While Al-Haj Mohamed's narrations were tape-recorded in Morocco, most of Fatima Zohra's were undertaken in the United States. If the context of biographical narration influences its nature, the same thing might be said about the way used to record it. So, while the author used a tape-recorder in his encounters with Al-Haj Mohamed, he confined himself to the direct inscription of his wife's biographical accounts; while both were permitted to narrate their own lives, the other members of the family were not allowed the same privilege.

If we add to all this the fact that the author included in the narratives data he had heard indirectly from them in the context of collective Moroccan discussions (Munson 1984), we will then be able to understand the extent of the writer's intervention in reconstructing his subject. It thus appears that the author's comparative approach was dictated more by the ideological background of the narra-

tive discourse than by its inevitable relationship with the context and techniques of its production.

Another interesting biography is Dale Eickelman's *Knowledge and Power in Morocco*. Its author aimed particularly to write a 'social biography' (Eickelman1985:14-15), revealing the general context into which religious learning in Morocco had evolved during the twentieth century. Along the way, he decided to study the personal and scholarly stages of the life of a rural judge living in Bzu, in the High Atlas mountains (Eickelman1985:16). Although the author occasionally permits Haj Abderrahman Mansuri to speak in his own words, Eickelman remains the principal speaker and interpreter. He intervenes constantly to incorporate social, economic, political and climatic events of the Protectorate and Independence periods, as well as to compare some aspects of Islamic learning in Morocco with those in other societies (Eickelman1985:58-59).

To write this biography, Eickelman had recourse to interview many of Haj Abderrahman's friends and relatives, as well as those he encountered while studying in Islamic educational institutions. But, at the same time, Eickelman did not rest only upon oral accounts. He also drew on personal and familial written documents that his informant had carefully conserved, and above all, relied on the Haj's personal diary (Eickelman1985:17, 26-30). He even consulted the newspaper Saada for supplementary information about Haj Abderrahman's marriage (Eickelman1985:128-129).

The author's main efforts were still directed to interviews with Haj Abderrahman, whose biographical conceptions were deeply impregnated by the Tarjama model (Eickelman1985:41-42). From the standpoint of what Eickelman considered to be essential knowledge, he led his informant to overcome what the 'tarjama' means as a formal presentation of self. For example, such accounts exclude women from the narration, and remain silent about the economic and political transactions that were undertaken by many men of learning, in such a way that the data Haj Abderrahman gave to the anthropologist were more abundant than those he had given to his own sons (Eickelman1985:34-38).

Nevertheless, by the questions he asked as well as by the later reconstruction in the text, the ethnographer's voice remains dominant. The social context of Islamic learning in Morocco is more the result of the ethnographer's interpretation and writing than a mere emanation of the informant. This does not mean, however, that the author has assimilated all the preconceptions he had learnt about the Middle East: 'In this study, I use a social biographical approach to break accepted stereotypes held by westerners and by many Middle Easterners themselves of Islamic learning and its carriers' (Eickelman 1985:15).

The second trend, which emphasizes above all the fieldwork encounter, the status of self and other, and the adoption of a new ethnographic writing experience, is represented by two American studies.

The first one is Kevin Dwyer's book, *Moroccan Dialogues*. What is relevant in this work from the standpoint of modern ethnographic writing is, first, the revealing of the extent to which dialogue is essential for ethnographic knowledge. The entire book is constructed as dialogues on specific events that the Moroccan interlocutor has to face (divorce, loss of a child, unhappy marriage of a daughter, circumcision, wedding, dealing with the police about a theft, etc). It provides, from the author's point of view, the occasion either to analyze the interaction with the Other, or to evaluate anthropological practices that have been taken for granted. It reveals, in addition, that the active role of the anthropologist appears more clearly in the interaction with the informant, than between the writer and his text. Only this dialectical encounter permits the production of the ethnographic text (Dwyer 1982:278-279).

Secondly, contrary to the epistemology which allows the anthropologist to reconstruct, partially or totally, the fieldwork experience, and to give primacy to the moment of interpretation hence keeping the reader distant from either the fieldwork experience or the ethnographic text, Kevin Dwyer attempts 'to bring the reader as close as possible to the experience' (Dwyer 1982:278-279) and presents to him the raw material of his dialogues, challenging him in this manner to participate in giving them the meaning he considers the most appropriate. And if he keeps his presence as interlocutor in the text, it is because he considers that objectivity is attained neither by hiding himself behind his informant nor by using the artefact of 'multiple biographies', but through the recognition of the researcher's subjectivity (Dwyer 1982:277). He recognizes, however, that the experience is inevitably transformed in making it into a text:

Events certainly lose their immediacy and are reworked in the mind of the writer as he writes them down much is lost in transcribing conversations into written dialogues: gestures do not appear, tone of voice is muted and mood is hidden, and Moroccan Arabic disappears as it is translated into English (Dwyer 1982).

Thirdly, the text is not presented either as definitive or as a model to be followed. Dwyer stresses rather the vulnerability of all participants in the ethnographic project: anthropologist, informant, and reader (Marcus, Fisher 1986:70).

The second book is Vincent Crapanzano's *Tuhami: Portrait of a Moroccan*, in which life history is presented as a puzzle, and with which the author consequently asks for the reader's help in interpretation (Marcus, Fisher 1986:72-75).

Tuhami refers in his narrative to the ordinary events of his life, as well as to fantastic metaphors (djnun [demons], magic shrines, angels, the legendary character of Atcha Kandis', etc). Such diverse matters as the fragility of childhood, the arbitrariness of desires, the manipulation of the woman, love, death, security, honour, shame, and dreams are expressed in different registers: the historical, the demonic, the magical and the folkloristic. The ethnographer aims through his encounter with Tuhami not only to have a general knowledge of colonial and post-independence period or to understand the characteristics of a cultural tradition, but also to determine attitudes vis-à-vis fundamental matters such as time, nature, the supernatural, the person and social relationships. Psychic processes and linguistic metaphors are dealt with by the author as valid means of communicating experiences. From his work with Tuhami, the author learned to distinguish between the personal history which equates narration with individual acts, and the truth of autobiography which exists only in the text. As anthropologist, he was convinced of the impossibility of having direct access to the mind of his interlocutor, and had therefore to seek it through the text mediation (Crapanzano-Tuhami 1980:5). He broke the traditional frame of history which was, in his opinion, largely influenced by novelists such as Balzac, Flaubert and Zola, to be rather closer to the form of the modern novel (Crapanzano-Tuhami 1980:10-11).

Tuhami's life history is not only informative but also evocative (Crapanzano-Tuhami 1980:14). That is why the author expresses his fear that interpretation may become over interpretation, and hence, invites the reader to be engaged in the process. But although he defines the life history as a process by which the subject presents himself from his own perspective, he recognizes, at the same time, the undeniable impact of the writing:

His text ... /Tuhami's text however accurately I can present it, is in a sense my text. I have assumed it and afforded myself as narrator, a privilege he has not been granted. I have had the privilege of (re) encounter. I hope, however, that through my assumption the reader will discover Tuhami and recognize in him something of himself (Crapanzano-Tuhami 1980:23).

Conclusion

The life account may serve as a vehicle for fictions, self invention and individual strategies. It is not synonymous with the truth about one's life. Even though the anthropologist can make fruitful uses of the illusions and lies it contains, the oral life account remains, in many of its aspects, confusing and ambiguous. Consequently, only new forms of ethnographic writing can lead to a better reading and understanding of life histories. Because the life account is so complex and peculiar, no single mode of writing can present an adequate transcription of it. What the anthropologist presents in his text is not the life

history, but only one version among other possible versions. While writing his text, he adds his own fictions to the narrator's fiction. What should be expected is an imperfect and vulnerable text whose meaning depends on all the participants in the ethnographic project: The anthropologist, the narrator and reader. None of them can claim for himself an exclusive privilege in producing the meaning of the ethnographic text.

On the other hand, because the narrator participates actively in interpreting his own life, and constructs it in a reiterative and progressive manner such, characteristics as process, reflexivity and understanding should be acknowledged not only in the social reality of the ethnographer, but also for individuals in the culture he studies. The split between ethnographer (as subject) and native (as object) should be overcome not only by the use of humanistic qualities such as 'sensitivity' and 'understanding', but also by the experimentation with new techniques for research and presentation of findings.

Note

1. The way John Waterbury and Dale Eickelman write biography seems very close to Lévi-Strauss' view, which consists of conceiving the life account not as a mere expression of an individual self, but as a sort of partial and weak history which cannot have any pertinent meaning outside a stronger and larger processes of history (Lévi-Strauss 1961:346-347).

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10

Audiovisual Instruments in Ethnographic Research

Clara Carvalho

Introduction

In 1973, the most renowned researchers in Visual Anthropology met at the ninth International Congress of Anthropology and Sociology to discuss the role of film and photography in ethnographic research and to systematize the almost century-old experiences of bringing together description, ethnography, photography and film. Opening the meeting, Dean Margaret Mead enthusiastically defended the use of audiovisual instruments in research. Considering that Anthropology explicitly or implicitly accepted the responsibility of ‘preserving’ the descriptions of disappearing cultural habits, Mead prescribed the need for an ‘urgent anthropology’ that used photographs and films as supporting research means. She endorsed the view that the camera should be used as a research instrument as well as a means of disseminating a study, conjuring concerns with the film’s stylistic and aesthetic qualities while stressing its documental value. In her own terms, Anthropology should maximize the potentialities of the audiovisual media within its reach instead of being merely a ‘science of words’ (Mead 1995).

Mead’s point of view was quickly criticized within a disciplinary field concerned with problems of interpretation of the subject, deep ethnographic description, and the post-colonial criticism of the role of the observer as a political subject and actor. The Danish anthropologist Kirsten Hastrup echoed these concerns when she denounced Mead’s positivist conception of the discipline. For Hastrup, the defence of an urgent anthropology could only be defended within the perspective of humanity as a ‘human zoo’, a collection of immutable and a-historical subjects and social formations that were better described, in their immobility, by pictures. On the contrary, Hastrup states that Anthropology is a form of knowledge made up of subjects engaged in an active relationship (‘observer’ and ‘subject’), historically situated and politically conscious. This interrelationship may

only be expressed by the textual 'deep description'. Furthermore, the creation of the visual archives of humanity, as defended by Mead, would be hostage to a view of the disciplinary field as engaged in preserving cultural habits that have become obsolete, refusing to recognize the historicity of the groups described. And she continues,

Anthropologists – as people – belong to the class of things that are subject to their understanding. Whether equipped with notebooks or cameras, ethnographers always define reality at the moment they discover it. It is this continuity between subject and object which marks the construction of ethnography and which must be taken into consideration when assessing the difference between visual and textual forms of authority (Hastrup 1992:10).

Visual Anthropology falls between these two opposite positions. It arises from a broad field of studies, which includes both local cultural manifestations such as visual creation, dance, aesthetics, architecture, as well as research records through audiovisual media, their edition and public presentation. Being an innovative mean of research and implying a different approach to the field, Visual Anthropology challenged the discipline itself. In this text, I will be concentrating on its use as a means of research and as a form of presenting a research project, ignoring the entire field of interpreting the material and visual manifestations of a defined group. The artificial distinction, made in this paper between the use of film (in film, video or digital format) and photography, is justified by their distinct histories within the disciplinary practice. My intention was not to carry out a systematic study on these media within the discipline but to discuss their potentialities and limitations. Finally, I will look at the use of complex digital products such as CD-ROMs and the Internet, which encourage new practices and new means of research diffusion.

The Use of Films in Anthropological Research

Both Anthropology as Photography and Cinema are forms of knowledge that arose from the industrial and academic development of the late nineteenth century and enlarged within the urgent need to classify, typologize and integrate human diversity into hegemonic political projects of the modern colonial period. In this sense, we may consider that the first films are ethnographic (*La Sortie des Usines*, *Le Petit Déjeuner du Bébé* by the Lumière brothers or the chrono-photographs of Félix-Louis Regnault), in the same way as the first systematic research in Anthropology resorted to the use of audiovisual instruments (such as the expedition to the Torres Strait of 1898, led by Alfred Cort Haddon). Currently, ethno-sociological documentaries, which have drawn closer to the language used in fiction movies and in television documentaries, mark our vision of ethnographic films. The work of Jean Rouch, French cineaste and anthropologist, author of the most innovative work in the field of ethnographic films, is in itself the best definition of this genre. Rouch, who anticipated and experimented within the

possibilities of ethno-sociological documentaries, considered Robert Flaherty and Dziga Vertov as the 'totemic ancestors' of Visual Anthropology. They certainly continue to be seen as the most prominent benchmarks of socio-anthropological documentaries. Robert Flaherty, a mining prospector operating in Hudson Bay, presented to the world the famous *Nanook of the North* in 1921, the delightful saga of the Inuit Nanook and his family in the Great North. Directed with the assistance of the people in the film, who intervened in the choice of the topic and the edited sequences, this narrative appears in the form of a narrative drawn between fiction and reality, the joint creation by a moviemaker and the people filmed. The arduous struggle of the Inuit to survive in the Great North is recreated in this enchanting work, where the actors involved chose to portray hunting and fishing techniques that were no longer used at the time so that the film could serve as a memorial of techniques dating back several millennia. The film is also worth watching for the expressive humanity revealed in the beautiful smile of Nanook, which led millions of spectators to identify themselves with this far-away hunter from the Great North, who died shortly after the beginning of this work. Moreover, this work is the product of an ethnological pre-survey and was directed at a form of human essence underlying the acts, representing Man's fight against Nature. In this film, we acknowledge the questions which Anthropology – and even Visual Anthropology – only raised seven decades later. How does one film others? What right do we have to manipulate and exhibit their image? As an image can be perceived by the senses, how does one involve the people depicted in the film in its creation? What relationship should be established with reality in a documentary film? *Nanook of the North* deals with these issues without losing sight of the fact that a film, as a form of information and entertainment, should maintain a recreational nature and should bring about an aesthetic emotion. The identification, the play on the emotions and on the senses made possible by the mimetic nature of the images, the narrative creation between those depicted in the film and the producers, have demarcated since then a style of ethnographic film that continues to be evoked to date.

Another great lesson from Visual Anthropology' 'totemic ancestors' comes from Dziga Vertov, Soviet producer and contemporary of Flaherty, whose cinematographic and essayistic work forces us to reflect on the manipulated character of every mimetic work. This reveals the true paradox of cinema which applies to Visual Anthropology, as Dziga Vertov puts it:

(Cinema) is, in fact, the product of the double work of men to organize and understand their existence and of the observer who puts together the images of this representation to take apart his own dynamic. Reality is neither the object shown, nor the constitution of the demonstration; it lies in the constant passage from one to the other during which the cineaste appears successively in the situation which he himself defined' (Vertov quoted in Piau 1991:149).

Dziga Vertov's work leads the spectator to reflect on the interpretation of realities and knowledge that the camera forces us to make. In a certain sense, he anticipates and responds to the critiques literary critic James Clifford addressed to the ethnographic texts in 1986, a summary of the reflexive critique of the eighties and nineties. Clifford stated that all ethnographic text is interpretive and expresses constraint of an academic, political, historical and even stylistic order. He was echoing Vertov when the latter affirmed the built-up character of the film, which only exists as a construction and interpretation of reality. His effort to create an interpretive film comes close to the *thick description* of Clifford Geertz, a description/interpretation that contributes to a multilevel understanding of social interaction. Between the notion of the film as an interpretive language in Vertov's work and Flaherty's fictional experience, the key elements of ethnographic cinema are drawn, affirming this media as a privileged means of presenting human experiences.

The potentialities of this media were not immediately perceived from within the discipline. The use of film and of photography in Anthropology was for a long time a secondary act to the discipline itself, despite always having its fans, defenders and practitioners. Between the two wars the world witnessed the development of documentary cinema and besides the works of Robert Flaherty and Dziga Vertov, previously mentioned, the movies by Jean Vigo, Joris Ivens, Jean Epstein and John Grierson are noteworthy. Also in Anthropology, trained academic were introducing cinematographic records in their research. In Mali, Marcel Griaule filmed *Au pays des Dogon* in 1935 and *Sous les Masques Noires* in 1938; Franz Boas filmed among the Kwakiutl in Canada, while Margaret Mead and Gregory Bateson used their camera and video camera in their research projects between Bali and New Guinea, taking 25,000 photos and shooting 6,000 meters of film as an example of their theories on non-verbal behaviour, comparing different cultures. For these authors, the camera was seen as another research instrument, whose potentialities for recording information were used independently of the final construction of an exhibition film.

After World War II, technological developments made it possible to address new potentialities in ethnographic film. Cameras become lighter with the dissemination of 16mm films, which led Jean Rouch to film without a tripod, with a camera on his shoulder, in close collaboration with the people being filmed. The main innovation lies, however, in the introduction of synchronic sound and light cameras in 1960, which made it possible to interview the people being filmed and to introduce their voice. Authors such as Jean Rouch invented 'cinéma vérité', later dubbed 'direct cinema', and the subtle boundaries between ethno-sociological films and fiction movies were softened. This was also the time of the emergence of the first centres dedicated to the production of ethnographic films in the academic arena. This approach promoted a close collaboration between anthropologists and movie directors, skills often present in the same person: one of the exponents of this tendency is represented by the work of Timothy Asch, Robert Gardner and Jay Ruby. In the sixties, several television chains were interested in this production and joint productions between academic and television

producers gave rise to several series disclosing ethnographic research. The best known is the Granada Centre in the University of Manchester, UK, where media series such as *Disappearing World* were produced. Furthermore, we witnessed the development of documentaries with typological concerns, centred on ethnomusicology, ethnolinguistics, technology and even rituals, as seen in Mead and Veuve's examples. This approach is also adopted by the IWF (Institut für den Wissenschaftlichen Film), founded in 1956 in Göttingen, Germany, which proclaims the superiority of films as a method for anthropological documentation and analysis. The IWF created the *Encyclopaedia Cinematographica* (EC), attempting to produce as many short films as possible (preference unedited sequences) on 'minimum units of human behavior' such as techniques and rituals, supposedly able to be compared with each other. These short films included written information such as the location of the group, date, or specific ethnographic information, but should not contain outside comments (Loizos 1994, 195-196). However, criticism of this policy of affective, political and interpretative stripping brought about a change of strategy both within the IWF and Visual Anthropology in general. Currently, ethnographic films have come very close to documental cinema and are both interpretative and thought-provoking. The ethno-sociological movie expresses both the restlessness of its author and the political conscious objectives of the people involved and filmed. It may even assume the media character as a means of political action, as expressed by the impact of films such as *The Kayapo out of the Forest*, by Michael Beckham and Terence Turner in 1989.

The designation *ethnographic film* indicates a wide variety of visual documents within the reach of both researchers and professional moviemakers. Its potential and dissemination were further enhanced by the dissemination of small digital cameras and the ease with which digital images can be worked on. The current low cost and high durability of digital cameras and recording media make it possible to use these machines as an effective support to every research. The so-called 'notebook cameras' have revealed new recording possibilities. Cameras can be used to capture research records, surveys of space or material culture, recording interviews or even staged activities such as technologies, dances, rituals and ceremonies. Both the researchers and the people who were filmed can work these recorded materials on. This allows sharing representations and even interpretations between the people involved, irrespective of how the records are used afterwards. The camera creates records that are mimetically *thick* and emotionally more powerful than traditional written annotations. Furthermore, movies use a language that is easier to share and disseminate, where the voice of the players and their collaboration in the ethnographic construction is obvious. Films are more easily shared than written annotations, and this potential enables a new dialogue between researchers and the people they work within their projects and who are not any longer considered as mere 'informers'. Finally, as mentioned below, these films may be shared via the Internet, providing research with greater transparency.

Photography and Anthropology

The intrinsic characteristics of photographic images deserve to be seen from a particular perspective. Still image is, before all else, a mimetic object. As an object it can be manipulated, collected, reinterpreted. As a mimetic body, as Susan Sontag reminds us, it is particularly effective in its apparent relationship of loyalty with reality. This relationship leads to a deceptive copy of reality, as it is based on the false premise that photographs are snapshots that do not take into account their manipulation, the setting, the choice of the moment, the point of view expressed. Sontag goes on to say:

To photograph is to appropriate the thing photographed. It means putting oneself into a certain relation to the world that feels like knowledge – and, therefore, like power (Sontag 1983:16).

Even if the photograph does not steal the soul, it manipulates an image and a self-representation, interfering with the fundamental rights of all human beings.

These characteristics – the creation of a manipulatable object with mimetic capacity and a power symbol – are present in all anthropological photographic collections since the nineteenth century. At the time, photography was used within the discipline as a form of objectively recording cultural and physical differences. An example of this is Alfred Cort Haddon's work, as mentioned above. Collections of pictures of submissive populations appeared in American and European museums in an exhibition on human diversity and of the technical superiority and power of the western photographers. Notice, however, that manipulating mimetic devices in order to create new symbols of power was a common way of experiencing the difference of social universes apparently irreducible in their cultural expression, as analysed by Michael Taussig in his inspiring work on the American Indians Cuna, *Mimesis and Alterity*. It is once again Sontag who brings to our attention the violent character of the act of photographing in her famous definition of photography as a firearm:

To photograph people is to violate them, by seeing them as they never see themselves, by having knowledge of them that they can never have; it turns people into objects that can be symbolically possessed. Just as a camera is a sublimation of the gun, to photograph someone is a subliminal murder (Sontag 1983:28).

This remark by the author has an obvious application in the construction of the first ethnographic picture collections that reflected the obviously manipulative and ideological construction allowed by the unshared image (Edwards 1992, Landau 2002, Ryan 1997).

Photography currently has different uses in ethnographic research. In the first place, it continues to be a vital instrument in the constitution of typologies, inventories, recording of cultural material, description and spaces and of the relation

between people and space, studies in proxemics, as defended by John Collier (1996). This media was also extensively used by authors Margaret Mead and Gregory Bateson who defended its 'scientific and realistic' capacities (Pink 2001:49). Furthermore, photography became a key element within the reflexive criticism of the eighties and nineties. The picture of Stephen Tyler on the cover of the iconic volume *Writing Culture* (1986) is the best example of this concern with the role of the researcher in the research process. Picturing the 'field' experience was seen as an illustration of the description of inter-subjective processes of the construction of knowledge within the research project.

Anthropologists also pondered on the social use of photography, influenced by the cultural studies. Photography is a form of representation, observing social rules significant in it. In the course of the work of Pierre Bourdieu, ethnographers studied family albums, considering photographs as a social institution that contributes to commemorate particular moments in the lives of the group. By analyzing the events pictured, the layout of the photographs in the album and their manipulation, researchers were able to identify their role in the creation of the group self-representation, as exemplified in works by Erno Kunt and Marianne Hirsch. Currently, new research has emerged on the construction of visual cultures, the local interpretation of iconic elements and the meaning of specific iconographies. Photography as a practice of representation is expressed in both social interaction and symbolic constructions, two of the key elements social research is addressing.

Even within the research process itself, photography can be used as a means of recording and exchanging information, ideas and representational practices. Both recording research moments, namely those which express the relationship with the people involved, and the act of giving back pictures to the people photographed, are means normally used to mark the relationship that was built up, as well as a form of retribution within the field research. However, photography can also be used to speak of self-representation practices, especially when the people pictured are involved in the construction of the photograph. Its classic use as a form of gathering elements (of material culture or events) is enriched if it is commented on by other members of the research team. It is also an effective means of giving rise to research, namely asking for clarifications on the people photographed or discussing the representations. Furthermore, it can be used as a subterfuge to understand events not witnessed by the researcher or which the latter does not understand. Above all, photography is a powerful means of building up relationships during the micro-focused research that characterizes Anthropology. The exchange of photographs, the act of offering them as a gift, the joint construction of photographic representations are invaluable means of sharing and building up a relationship while the research is being carried out (Pink 2001).

New Technologies

The use of CD-ROMs, web pages and even blogs has raised new research possibilities especially related to the dissemination of the research products. These were announced by the dissemination of the CD-ROM and of hypertext, a means of expression that made it possible to combine different types of materials, such as textual production, films, photographs or other iconographic sources, sound, including the voice of interviewees during the research process. These characteristics transform hypertext into a medium that is particularly adjusted to the diversity of ethnographic research, where there are countless meaningful elements. By including the image and voice of the people who took part in the research, the latter are given the role of co-authors of the final product, recognizing their own voice and differentiating it from that of the researcher. The identification of the various media and of the different texts makes it possible to separate the analytical interpretation from the description or even from the data gathered (Sperber 1982). This medium responds to the questions put forward by Margaret Mead and Kirsten Hastrup that were mentioned in the beginning of the text. This new medium includes the theoretical explanation of the text, which Hastrup associates with the density of written ethnography, enriching it with the multiple possibilities of the audiovisual surveys extolled by Mead.

Hypertext also calls for non-linear reading and a deeper and more active involvement of the reader, who is free to choose the materials presented on which to construct his own representation. As such, it leads to a less directive approach than the written text, but of a greater organizational complexity. Authors such as José Silva Ribeiro and Sérgio Bairon, or Rod Coover, have demonstrated the potentialities of this means of research diffusion, with particularly appealing results.

The characteristics of the CD-ROM were updated by the dissemination of web pages. Like CD-ROMs, web pages are characterized by the diversity of means and by the non-linear aspect of their presentation. Added to this is the possibility of the site being shared, commented on or even changed by a large number of readers in completely different locations. Anthropology, which Clifford accused of limiting itself to the academic environment, finds here a freer form of diffusion that is easier to read by the public in general and by interested parties in particular. Experiments worth mentioning are those conducted by the University of Kent (UK) on the sharing of information, particularly the Mandilla project headed by David Zeitlyn (available at <http://lucy.ukc.ac.uk/dz/>) or the work of Akos Ostor and Linda Fruzzetti in India. The possibilities of sharing information are endless, making it possible to set up interactive blogs and Internet sites. Images are an essential part of these media, where photographs and filmed sequences are thrown into relief. When the presentation is based on multiple texts, the filmed sequences are generally not very complex and it is easy to add new research data such as interview extracts or short surveys. Photographs are particularly suitable

to this medium and may be used in any of the versions referred to above. The researcher must, however, respect the ethical principles of sharing images and other representations, involving the people photographed in this presentation of the research. The representational, interactive and sharing potential of the Internet has brought about a new form of diffusion for ethno-sociological research, decisively distancing Anthropology from the 'science of words' ascribed to it by Margaret Mead.

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11

Establishing an Observation, Producing a Discourse, Illustrating Results Possibilities for the Photographic Tool

Anne Attané

“The social sciences should construct image-tools which will enable them to understand, analyze and explain reality in its visible aspects. They should also specify the approaches to be used to render an account of ordinary or extraordinary subject-images produced daily in the social universe” (Terrenoire 1985:512).

Starting from three concrete examples, we will underline several potentialities of the photographic tool for study in the social sciences.¹ Various research experiences have shown me that the photograph can be both a tool in the development of the anthropological subject and throughout the research process, but also a way to present the research results.

Photography and ethnography appeared at the same time and shared the wish to describe and understand social life. The use of photography in the social sciences is a long tradition.² Anthropologists have often had recourse to photography without necessarily theorizing about its use. The first important work using photography as a tool of research is that which Margaret Mead and Gregory Bateson conducted in Bali at the beginning of the 1940s. Using their photographs, they tried to determine a Balinese type.³ Generally, photography is used to illustrate the anthropological text during the reproduction of research results. Some African researchers have, for example, conducted research on images produced in African societies to analyze changes in social relationships⁴ or colonial history.⁵

Work using photography starting from the production of data is much rarer.⁶ Generally speaking, the social sciences grudgingly recognize that an image might have a scientific quality. Because the image can produce impressions, express feelings, it appears frivolous and its use as a research tool seems suspect.⁷ However,

the image is a sign of the real. And as Albert Piette writes, “as an irrefutable witness of the existence of the photographed object, the photographic image contains a power of designation which endows it with a basic heuristic quality: to show, attract our attention to an object, a theme...” (Piette 1992:26). But even the very people who recognize the importance of the image still miss no chance to regularly remind us of the limits of photography.⁸

The Limits of the Photographic Tool

A photograph obviously depends on the choice of the photographer and his or her interpretation. It reflects what the photographer, in a more or less conscious way, wanted to see and show of this reality. One of the dangers of the use of the photograph is the realistic illusion. The photograph represents the direct proof of the presence of the anthropologist on the ground and gives a character of truth to his or her interpretations. Writing also creates this realistic illusion when anthropologists stress their presence on the ground.⁹

Beyond this aspect, the use of the photograph poses a major problem: the polysemy of an image. The photograph is not a linear discourse; the picture has its own peculiar language, the codes of which are shifting depending upon the person looking at it. The esthetic codes, the recognition or misreading of the objects presented, the affective relationships of the person who is looking at what is represented, etc. result in each individual perceiving and feeling different realities – or at least not totally identical realities – in discovering the picture. For this essential reason, in terms of communication, the photograph is very weak. Ernst Gombrich affirms that “the visual image is unequalled with respect to its ability for discovery, its use for expressive ends is problematic and when reduced to itself, the possibility of equally the enunciative function of language is severely lacking” (Gombrich 1983:324). If the image wants to retain a power of communication, it should at least be accompanied by a text.¹⁰ According to Gombrich, the text is not a sufficient condition; the true interpretation of an image is not a passive act. To his mind, “a correct interpretation depends on three variables: the code, the legend and the context. One might think that the legend would suffice to make the two others superfluous. But our cultural conventions are too supple for this to be the case” (Gombrich 1983:328). Indeed, we can apply various readings to the same picture which change its meaning. In some cases, only the context can clear up the ambiguity of a visual message even without resorting to words.

The translation of a context seems possible by viewing photographs in a series. Mastery of the photographic technique also allows for a visual translation of the context. According to Guran, a photograph can have a strong communicative power, if it is effective. For this to be the case,

the photographic act begins with the recognition of the content of a scene, the selection of the aspect which is worthy of being recorded. In the viewfinder, we exclude, or not, certain elements – which, however, also

represent data – in order to bring out the essential part of the scene according to the viewpoint chosen. It is totally fundamental to look carefully at the four corners of the viewfinder and to eliminate the maximum number of incidental elements which could pollute the main message (...) [Without doing so] the effectiveness of the communication of the photograph is reduced by the presence of disorganized visual elements. (...) The photographic image is built from the visual element which represents the starting point of its reading. This point should be recognized from the first glance at the photograph. It is the first visual element which attracts our attention, everyone is supposed to begin the reading of the picture from this. The absence of that point, or the existence of several equally prominent points can be an esthetic approach, but in general, with respect to grasping information, makes the picture confused and weak (Guran 1996:367).

For Guran,

the effective photograph does not have the objective of rendering the text useless, but only of creating a photograph which contains more meaning and which is likely to convey it more easily. However, the effectiveness of a photograph cannot always be considered at the very moment of taking the picture. It is often discovered *a posteriori*, just as an effective datum is, once the field work completed and the notes considered in their entirety. Even if the photographer fully masters the codes of photographic language, all of his intention is not contained in the photo; he cannot guarantee that the receiver sees all that he wanted to put in it (Darbon 1994:115).

Semiology has shown the need to distinguish between the transmitter and the receiver; this is true for a text and even more so for photographic language.

The need to master photographic language in order to produce photographs which are tools of research requires the anthropologist to be a photographer himself or to work with a photographer. In the latter case, real team work is necessary to collect visual data and exploit them.

Anthropologist-photographers ¹¹ say it themselves: it is difficult, if not impossible, for a single person to juggle anthropological inquiries and photographic work with the objective of producing a corpus of scientific data. The observation of a ceremony, for example, cannot be done both with the objective of producing a precise and systematic written description and extracting photographic material from it. These two jobs, very different in nature, should be accomplished at different times and according to specific modalities. From this viewpoint, working in a team allows for getting around this difficulty. The constraints and approach of the photographer, however, are not the same as those of the anthropologist. For the photographer, the issue is “to foresee (or better yet to have the intuition) and to capture a representative synthetic moment of the universe being studied. All of this means that the photograph is the product of a strictly personal act, the direct result of the interaction between the

photographer and the content of the recorded scene” (Guran 1996:364). The photographer should keep in mind the various aspects of the use in the social sciences of the visual support (to describe, compare, illustrate an interpretation, to represent actors...) The photographer should immerse him or herself in the questions of the social science researcher, and the anthropologist should be conscious of the constraints of photographic work which is the result of the thought and point of departure of another. Time for photographic work and that of anthropological thought is not the same. The anthropological subject is constructed little by little during reading and research in the field, based on a complex back-and-forth between the two, the final result requires several months of reflection. Certainly, the photograph produced for the social sciences requires a certain immersion of its author in the universe under study, but the result itself is immediate.

Team work allows the ethnographer to observe the photographer at work. He or she can then reflect on what might be significant on the visual level. This collaboration offers the ethnographer the chance to compare his approach and study in progress from a different angle which assumes a different sort of questioning. For the anthropologist, the fact of following the photographer on the ground represents a privileged means of observation, all the while maintaining the distance necessary for the understanding of certain phenomena. The anthropologist observes while the photographer goes to another support with his or her own sensibility, to observe aspects sometimes already seen, sometimes new. The presence of the anthropologist during photographic work is necessary for the understanding and analysis of the interaction between the photographer and the photographed and understanding of the off-camera. Scientific study is significantly enriched by all of these elements.

Photography, we should recall, depends on the selective interpretation of the photographer (choice of angles of view, distance to the object, framing). “We know that it is not the camera that takes the photos, but instead the photographer who introduces his way of perceiving and constructing the world according to subjective determinations (...). In short, the image is not the real” (Piette 1992:28). Photography remains an interpretation. The anthropologist, like the photographer, should be aware of this. If the anthropologist is not the photographer, he or she can more easily distance him or herself from the interpretation of the photographer and think about it.

Here, we are not in any way concerned with denying the limits of the fixed image, but rather of knowing how, despite its shortcomings, the image can be a supplementary tool in the panoply of the social science researcher at the various stages of his or her reflection, i.e., during data production and the reproduction of results and, of course, in the interpretation inherent in understanding the world. The production of images is one tool among many others; it is in no way designed to replace the written description of phenomena observed.

Photography captures specific data which are often difficult to render in writing. The obligatory criteria to which the photograph may be subjected can be as strict as those used for writing in the social sciences. The image can in no way replace discursive production but, when realized according to specific and rigorous modalities, it provides another regard which calls upon us to fully consider the human dimension in our subjects of research.

Most of the images presented here were produced by a photographer, Franck Pourcel, with whom I conducted three anthropological research projects. The first of these projects was an outdoor study in two French industrial cities, Martigues and Cherbourg. The second, conducted in collaboration with another anthropologist, Katrin Langewiesche, dealt with alternative life styles, developed by people who are currently referred to as “neo-rurals” in France. These are people who, mainly after 1968, returned to nature. Whether they live in a community, alone, or in families, the common element is their desire to attempt a new experiment in living on the fringes of consumer society. The inquiry undertaken was neither a classical ethnological study nor conventional photographic research, but a work which combines these two approaches. (See Attané, Langewiesche, Pourcel 2004). Finally, the third experiment in collaboration took place in Burkina Faso during my doctoral research which dealt with family ceremonies and the changes in social relationships of sex, age and generation. At the end of my twelve months on the ground, Franck met me for a month and took pictures of marriage ceremonies, naming ceremonies, and funerals. After Franck Purcell’s departure, Abdoulaye Ouédraogo, who accompanied me in my field work, took tens of pictures during the funeral of a religious leader in the village of Bougounam. Informed by anthropological questioning, his photographs are aimed at reconstructing the stages of the ritual. The photographs taken in Burkina Faso did not contribute to the creation of the subject as was the case during my two other experiences in the field, but they enabled me to pay closer attention to certain data. To this extent, they contributed to the emergence of certain analyses. At the same time, they helped in the reproduction of descriptive, comparative or interpretive data thus contributing to the illustration of research results.

These three research experiences show that photography can be used in anthropology on three distinct levels:

1. First, photography can contribute to the construction of the research subject; such was the case during the study of outdoor activities;
2. During the research process, photography can be a full-fledged research tool. It supplements description and comparison. Later, when presented to the actors, photographs contribute to the collection of a discourse on questions which could only with difficulty be organized by themes during the interview;

3. In the reproduction of results, photography becomes an illustration of research. During these three experiences, the use of photography allowed for the presentation of visual data which, if they had been transcribed linearly, would have lost a part of their meaning. These visual data illustrate in turn a description, a comparison or an interpretation (interpretive illustration).

Sometime, a single photograph can be used at each of these levels, whereas another can only be used for one. A photograph can be read alone, but it is the entirety of the photographic corpus which clears the way for the meaning of the visual discourse.

The Construction of the Subject of Research

The image participated in the construction of the research subject in the framework of a comparative study of practices of the environment in the two French cities of Martigues and Cherbourg. Here is a first picture which was taken in the hamlet of Ponteau in the district of Martigues, next to the Mediterranean, one Thursday in June. It was warm; a young woman was sunbathing in front of the huts on the coast.



1. Village of Ponteau (Martigues, June 1996). Photograph: Franck Pourcel

By turning, we can see the chimneys of the petrochemical factories which overhang the huts can be seen ; the photographer gives us a glimpse, once he was able to capture a panoramic view of the site from the other side of the conche.



2. Village of Ponteau (Martigues, June 1996). Photograph: Franck Pourcel

At the same time, a person I was talking to that I ran into on the ground that day exclaimed, “This spot is magnificent! Do you know many places like this on the Mediterranean which are as calm and where there is no crowd?”

This example allows us to grasp the gap which exists between what the external observer can see of an activity and what an actor can say about it. The photograph illustrates this gap and, at the same time, envisaged alongside the discourse, it becomes a datum for reflection for the social sciences. Here, we are not contrasting the discourse of interlocutors and information captured by the photograph (here: a tanned woman, lying on a deckchair, not far from a factory), but rather analyzing them together as two facets of the same reality. On the one hand, an actor produces a discourse on a place, and on the other, an external observer – the photographer – captures what seems surprising and important to him in the reality observed: the fact of devoting one’s time to a leisure activity close to a factory. The photographer’s look at the activity of an actor gives rise to sociological questions. What are the representations of the space of the actor? What motivates his or her choice of location for leisure activities? What are the modalities that cause a single space to be perceived in very different ways by its users? This rendered by the photographic document, also requires us to ask about the presuppositions in play in any observation of social reality. The recourse to an image at each of the stages of research prompts us both to take the new data into account and to reflect on data production in anthropology.

These cities of Cherbourg and Martigues are hosts to so-called at-risk industries: the nuclear industry in Cherbourg and the petrochemical industry in Martigues. The

very different nature of the industries implanted just as the cultural specificity of these two cities justifies the comparison. In Martigues, the omnipresence of industrial installations strikes the visitor. In Cherbourg, despite the significance of installations using nuclear energy (the city's arsenal, the factory for radioactive waste reprocessing in La Hague, the nuclear plant in Flamanville), the countryside maintains a preserved and "natural" look. The idea of using photography was therefore obvious to represent the specificity of each site. The photographer and the anthropologist wondered about the possibility of producing visual data enabling us to establish comparisons. This questioning underlines the fact that the industrial installation in Cherbourg was not very visible and, as such, was very difficult to represent on a photographic support. The invisibility of the nuclear industry was then obvious as an incontrovertible fact in data analysis. The creation of a comparable visual corpus produced information which contributed to the construction of the anthropological subject. Thus, even before their existence, the possibility of taking pictures or not contributed to our reflection.

One of the aspects of research was to capture outdoor activities in their setting, which the photographer set out systematically to do. After the first investigations (interviews, observations and production of negatives) the existence of two different attitudes vis-à-vis the industrial facilities appeared. On the one hand, in Cherbourg, of process of shunning nuclear facilities was discernable both in discourses, visible in observations and rendered in images; and on the other hand, in Martigues, a sort of invisibility of industries seemed to characterize all of the outdoor activities. Indeed, even though factories cover all of the territory, the discourse of the people encountered would lead us to believe that they are not seen.

The Tool of Photography in the Research Process

The tool of photography can also be used in the research process. It assists in the production of descriptive data.

Description

The photographic support facilitates description for three main reasons. First, it allows us to capture a visual observation; secondly, it establishes a multitude of details that direct observation does not necessarily take into account; thirdly, it allows us to immortalize an instant and the perception of things that the photographer had of the situation at this instant. In this sense, it allows us to transform an impression into data. But a description, whether it be visual or textual, cannot be separated from the interpretation of the researcher, conditions of its productions and its recipients.¹² Thus, the photograph reflects the choices of the photographer; it is partial and biased. The anthropologist must consider the interpretive dimensions of the image. The photograph in its role as the trace of the real also has a strong realistic power. The anthropologist reflects on its realistic effects, i.e. on the information conveyed by the image (clothing of the actors, body language, background, landscape, etc.).

The photograph can facilitate description in the research process because it allows us to visually understand a part of the phenomena observed. Within a study of outdoor activities, the photograph captures data which would be difficult to represent without this support: description of the setting of cities and their surroundings, description of the immediate environment of the interlocutors. In Martigues, the proximity of the industrial facilities strikes the visitor. The chimneys of the factory in Ponteau seem to watch over the near-by houses. Factories are omnipresent; they mark the scenes of daily life.



3. Hamlet of Ponteau (Martigues, July 1996). Photograph: Franck Pourcel

The presence of industries marks the landscape of the village as a whole. The homes in the village of Lavéra are right next to petrochemical facilities.



4, 5 and 6. Hamlet of Ponteau (Martigues, July 1996). Photograph: Franck Pourcel



7. Hamlet of Lavéra (Martigues, July 1996). Photograph: Franck Pourcel

The great visibility of petrochemical industries shown in the photographs and, at the same time their absence in the discourse of the interlocutors, gives rise to specific interpretations. We are aware of the development of the process of rendering the factories invisible and minimal by residents near the industrial sites. The visual description is placed next to the discourse. We thus recognize the process of rendering invisible and minimizing created by the neighbors of the industrial. Visual description placed next to discourse is of indisputable heuristic effectiveness in this case.

The second reason why we are encouraged to use photography for ethnographic description is that the support can record a multitude of details.¹³ Thus when the observer needs to note a great number of elements inscribed in the scene observed, the photograph is an essential tool. This is the case, for example, in this image which represents the outside of the house of one of the people whom we met in conducting research on the alternative lifestyle in south-west France.



8. Ponny in front of his house (Forcalquier region, September 1998).

Photograph: Franck Pourcel

As Albert Piette describes, by allowing for “note-taking, potentially on everything in the world from all possible angles” (Piette 1992:27), the photograph facilitates a detailed description.



9. Photograph taken in Ponteau (Martigues, July 1996). Photograph: Franck Pourcel



10. Photograph taken beside pond in Berre
(Martigues, November 1996). Photograph: Franck Pourcel

Thus, in the study of outdoor activities in Martigues, the photographs highlight the behavior: the fishermen, the swimmers bring chairs, food, etc. The nearby car hides the thousand and one familiar accessories. We receive information directly, thanks to the tool of photography, on certain aspects of social life. This possibility for the image to take in a multitude of details in a single shot is particularly interesting to be able to describe the details of a ritual practice.

Finally, the photograph facilitates description thanks to its ability to immortalize an instant. Pierre Verget wrote that it “has the advantage of stopping things... and thus enabling us to see what was only glimpsed at and immediately forgotten because a new impression came to erase the previous one” (Verger 1991:168). The image of a Catholic marriage taken in the village of Ouahigouya in Burkina Faso illustrates perfectly this potentiality of photography.



11. Ouahigouya (Burkina Faso, February 1997). Photograph: Franck Pourcel

This is also the case with respect to the photographs taken in Martigues: the near-by factories are visible from almost everywhere. The visual violence of the industrial facility leaves the visitor with a mixed feeling. By capturing the moment, the fixed image allows us to turn a vague impression into a fact to be examined. The photograph does not accomplish this less well than film; it does so differently. It is not a handicapped image. With the photograph, the movement is not reconstituted but stopped. For example, these photographs mark practices of the environment in their setting:



12. Hamlet of Ponteau (Martigues, June 1996). Photograph: Franck Pourcel



13. Caronte Canal (Martigues, July 1996). Photograph: Franck Pourcel.

In the study of ritual or social practices, the context must be taken into consideration. During research conducted in Martigues and Cherbourg, capturing the context was essential because an outdoor activity does not have the same meaning if it takes place in a natural park or next to an oil port. Because the dominant social norm imposes a “natural” framework for outside activities, these photographs show us activities which by-pass this norm. Direct observation may be sufficient to perceive the context, but the photograph allows us to consider it at each stage in the research. The photograph then becomes a primordial tool for “an ethnography of the unspoken, of that which is not admitted, most often in an awkward position with respect to the discourse” (Piette, 1992:33) and I would add the “unseen.” Photographs, the result of an external regard, can show elements of a landscape or an activity which are not necessarily seen by the actor.

Comparison

Images can also contribute to the production of comparative data in the research process. Thus, for example, during the research conducted on the alternative lifestyle, Katrin Langewiesche, Franck Pourcel and I realized that the individuals who attempted this return to nature are from various social milieus. While they shared common ideas on organic agriculture, therapeutic choices, or children’s education, these people developed different attitudes as they were confronted with a consumer society and chose to live in a more or less significant material destitution. The following two pictures of these two men taking care of their garden illustrate this point.



14 and 15. District of Mane (Forcalquier Region, June 1999). Photograph: Franck Pourcel

All of the pictures taken in Martigues and Cherbourg provide the opportunity to examine, side by side, the environment of the two villages and the relationship that the actors have with their space. The visibility of industry in Martigues is striking, whereas in Cherbourg, the countryside and green space are very present visually. In the same glance, in Cherbourg, we only rarely see industrial activities and use of space which might be termed “natural.” In Martigues, on the other hand, people swim, eat, live right near the factories.



16. Collignon Beach (Cherbourg, August 1996). Photograph: Franck Pourcel



17. Bay of Ecalgrains (Cherbourg Region, August 1996). Photograph: Franck Pourcel

The desire to produce visual data which add to the description of the research universe and the comparison of actions and places has encouraged what Jean Pierre Olivier de Sardan calls the production of “interpretive models coming from the field.”¹⁴

A Photograph for a Discourse

During the research process, the photograph can be used during interviews as a basis for posing questions. The main detractors to the use of photography in the social sciences criticize precisely its shortcomings in terms of communication. Esthetic codes, the knowledge or lack of knowledge of objects presented, the affective relationship of the person who is looking with what is represented, etc. mean that each individual will perceive and experience different realities – or at least not totally identical – in observing the image. It is for this essential reason that photography is a weak tool of communication. The polysemy of an image means that it can give rise to multiple interpretations, and it is for this reason that its use during interviews is rich in information. The view of images facilitates the production of original discourse. Two out of three examples of research presented here provided the chance to show images during interviews with our interlocutors.

Some photographs taken in the neo-rural movement were used systematically as a starting point to ask questions during interviews. Discussions based on the photographs enabled us to compile a corpus of very specific data on representations of landscapes and activities related to nature. Thanks to the photographs, the impressions of the interested parties are transformed into sociological handleable data. The discourse that the view of the photographs evokes proves to be rich in representations of the relationship to space, motivations of the actors in their choices. During the interviews, the images encouraged discussion on themes that might be difficult to put into words (because they are unconscious or hidden). Thanks to its evocative ability, photography translates emotions that the discourse of the ethnologist cannot render in the course of an interview. Finally, photography renders sociological questioning accessible to people encountered in the research setting and facilitates understanding between researcher and interlocutors. This was the case with the following photograph.



18. Skeleton of a goat (Hill in Mane, Forcalquier, June 1999). Photograph: Franck Pourcel

This image refers directly to death, and, at first glance, it does not really reveal an ethnographic meaning. For the photographer, this image represents a form of anchoring; for the ethnographer, it is a signature photograph which, although it presents the sensitivity and individual interpretation of the photographer, did not provide ethnographic data. It was later, when this photograph was shown to some of our interlocutors that its ability to elicit discourse appeared. Not a single one of the people met was indifferent to this photograph. Upon seeing this photo, they spontaneously developed a discourse on the installation in the region. For all, the image symbolized the approach of a number of them who came to settle and build a house. It was a chance for them to evoke the hard labor of construction, but also the affective and symbolic importance attached to the house or break-up of the couple which often happens when the house is finished. This signature photograph is useful for specifying what cannot be shown; information not evoked in the interview because it is too affective. Just as they capture information, photographs translate emotions; they thus have an ability of evocation that the discourse of the anthropologist during the interview cannot evoke. Here, each person evoked the significance attached to the house and the fact of reconstructing it oneself.

About ten images were also presented to interlocutors in Cherbourg. Having recourse to photographs in the interviewed proved to be relevant for five main reasons.

1. The corpus of the discourse collected thanks to photographs was the subject of specific treatment. It allowed for a comparison of what various actors were able to say about the same picture.
2. Photographs visualize certain elements of the research subject. As a result, they provide the actors with the result of an observation made with the objective of scientific questioning. Thanks to photography, the concern of the researcher was able to be better understood by his or her interlocutors, even if they interpreted the picture based on their own referents and not on those of the researcher. Starting with their knowledge or what they wanted to say about the scene represented, the persons interviewed developed a discourse: in this sense, photography has an indisputable heuristic power.
3. Photographs establish a situation and contribute a visual description of a place or an action in the setting. As a result, they were the origin of a set of discussions on a specific activity or space.



19. Port of Cherbourg (August 1996). Photograph: Franck Pourcel

This photograph provoked reflections by the Cherbourg residents encountered on fishing in the region, on its economic role, but also on the difficulty of working as a professional fisherman. Thanks to this image, it was possible to assess the position of the interlocutors with respect to this activity. The people encountered evoked new themes which did not appear in the interviews.

4. Once captured by photographs, unspoken or unseen information can be revealed to the actors. The discourse, sparked by the view of the photographs, turned out to be a wealth of representations of the relationship to space, of the relationship to the other and also in justifications constructed *a posteriori*. In Cherbourg, whether one is pro or anti nuclear, no one is indifferent to the presence of nuclear power. The existence of this industry in the region is the subject of stories and jokes of all sorts which reveal the effort at acceptance¹⁵ of the existence of a potential danger. Those in favor of the nuclear presence generally produce in technical discourse. This was the case of an employee of the radioactive waste reprocessing factory in La Hague who affirmed his confidence in the industry and the insignificance of the risk he runs in working there.



20. Photograph taken in front of the factory in La Hague (north Cotentin, August 1996).

Photograph: Franck Pourcel

Later, during the discussion, when looking at this photograph in front of the La Hague factory, he states: “The factory is half the size of Chernobyl” After a silence, he adds, as if to explain himself, “And there are animals that are not in a good shape. If you show the photo to someone who doesn’t know the region, they would say it was Chernobyl!” By presenting the vision of an outside observer, the photograph set off – beyond a rational and trivializing discourse – the expression of an anguish provoked by the presence of the site. Only a long time on the ground and particularly close contact with the interlocutors would have allowed us to find the ambivalence of feelings produced by the existence of the site, even among its most passionate defenders.



21. Bay of Écalgrains, Cherbourg Region (August 1996). Photograph: Franck Pourcel

The view of this image of the Bay of Écalgrains (very close to the factory for reprocessing radioactive waste in La Hague) was the occasion for a discourse on the beauty of the landscape. It enabled us to assess the attachment of the people encountered to the preserved and natural aspect of their environment.



22. Beach in the Village of Gatteville, near Cherbourg (Cotentin, August 1996)



23. Beach in the Village of Gatteville, near Cherbourg (Cotentin, August 1996)

5. The artistic dimension of the photograph has its importance because as it captures information, it also translates emotions. The photograph thus has the ability of evocation that the discourse of the anthropologist cannot have during an interview. The role of an activity in the environment in the history or the socialization of an individual, that of the relationship to the presence of an at-risk site, that of the anguish caused by the proximity to nuclear facilities can be more easily addressed. We see an example here:

Upon seeing these photographs, a man met in Cherbourg declared, pensively: "It's funny because there is a timeless side to this photograph; it could have been taken ten or twenty years ago." Another person adds: "For me (whelking) is a culture, a culture in the area, a tradition. Because when I was a kid, everyone always taught me to whelk."

Another interlocutor declared enthusiastically: "Your photos are good because everyone can see his or her life in them!" Precisely, because the photograph has this awakening power, it allowed us to examine the role that whelking played in the socialization of individuals and their relationship to natural space.

Presentation of Research Results

The photographs presented here reproduce a part of the research results. One by one they illustrate the descriptions, comparisons or interpretations. The polysemy of an image is an asset when it is presented during an interview; on the other hand, it becomes a limit during the reproduction of results. If it is to retain a power of communication, an image should be accompanied by a text.¹⁶ The text is not a sufficient condition because the interpretation of an image is not a passive act.¹⁷ Indeed, we can apply different wordings to the same image which can change the reading of it. In some cases, only the context can remove the ambiguity of a visual message. Understanding of the context of the photographed image is essential to correctly interpret an image. Several techniques facilitate the representation of the context: the construction of the image allowed by mastery of the photographic technique, the legend (cf. Gombrich, 1983) of the series of images (cf. Attané, Langewiesche, Pourcel, 2004).

Sometimes, the construction of a shot is enough to provide a correct interpretation of the image. The example of the fishermen at the Caronte Canal in Martigues shows how the construction of an image can situate the context of the instant captured.



24. Caronte Canal (Martigues, June 1996)

In the middle ground, two men are sitting side by side on the rocks. Farther off, fishermen are holding their poles. Here, the mastery of the photographic technique constructs the subject.¹⁸ The photographer, using a wide angle (35 mm), returns human action to its context, its environment. This is not a portrait: the desire is not to capture the regard or the individuality (the man in the foreground is seen from the rear), but rather the action of this man in his immediate environment. The photograph contains sufficient element to read it, even beyond the instant it represents *même de l'instant qu'elle représente*. This photograph may be termed "effective" because it has a significant power of communication. The "effective" photograph is not designed to render the text useless, but only to create a representation which contains a meaning which it easily transmits. The "effective" photograph is one which, standing on its own, provides sufficient information on the context of the instant photographed. Its use in the representation of results increases their relevance.

The legend, i.e. a written commentary about the image, is most often necessary to allow the viewer to understand what its user means to say about it. This is, for example, the case in the following images which show a part of the funeral of a religious leader, a master of the earth, which were taken in Burkina Faso by Abdoulaye Ouédraogo. Here are the commentaries which precede the presentations of these images in my thesis, commentaries which are indispensable to the understanding of the scene depicted. This example shows how images can illustrate descriptions and facilitate the reproduction of research results. Here, the visual and discursive descriptions are closely intertwined.

In Bougounam, during funerals of the *tengsoba* (master of the earth) of the village around 2:30 P.M., the men are in front of the house of the ancestors (the *kiims-roogo*) which is located inside the courtyard of the *tengsoba*. They surround and wrap the tombstone in a blanket (blanket made of black and white cotton fabric). The women of the deceased's family and the *tengspoko* stand facing the men.



25. Men build a stretcher to carry the tombstone with the image of the body of the deceased during the burial.
Photograph: Abdoulaye Ouédraogo



26. Village of Bougounam, Burkina Faso (March 1997).
Photograph: Abdoulaye Ouédraogo



27. Village of Bougounam, Burkina Faso (March 1997).
Photograph: Abdoulaye Ouédraogo



28. Village of Bougounam, Burkina Faso (March 1997).
Photograph: Abdoulaye Ouédraogo



29. Men and women leave the courtyard and walk around it three times counter-clockwise.
Photograph: Abdoulaye Ouédraogo



30. Women, parentes à plaisanterie [social practice consisting of a joking relationship between people to mitigate potential social tension], walk around in the opposite direction and pass them three times.

Photograph: Abdoulaye Ouédraogo

A woman, a elder sister of the deceased, opens the procession, she sprays water all along the path, other women follow, one of them carrying millet beer in a little crock, another a basket. The *wemdamba*, i.e., the women in the family of the deceased, are in front and the men follow. The *tengspoko* is there; she is wearing a red hat and white clothing.



31.



32.



33.



34.

31, 32, 33 and 34. The cortege heads towards the cemetery.
Photograph: Abdoulaye Ouédraogo

Everyone follows the group. The numerous children rush ahead and encircle the cortege. Now and then, the elders tell them to move away but they very quickly cluster together again. The carrying of the tombstone is much less solemn and formal that it would be in other villages.



35.



36.

35 and 36. The procession continues to the cemetery.
Photograph: Abdoulaye Ouédraogo



37. Ouahigouya (Burkina Faso, February 1997). Photograph: Franck Pourcel

These photographs show the importance of the role of the sisters of the deceased during the animist funeral rituals in the Mossi culture in Burkina Faso. They emphasize a certain modality of relationship between the sexes during the ritual which brings together hundreds of villagers (Attané 2003). In this way, they illustrate a sort of anthropological reflection. The series of photographs is necessary to limit the scope of possible interpretations. The written description is a representation of the reality (cf. Sperber, 1982: 18), the photograph itself is a representation that the photographer intends to provide of this same reality. Ethnography can be considerably enriched by combining these two representations of reality.

Conclusion

In the three pieces of research presented here, the photographic tool contributed additional elements of reflection which contributed to fleshing out the anthropological subject. First, the questioning before the shots were taken produced data and interpretations. For the anthropologist, watching the photograph work encouraged the confrontation of points of view. The ways of understanding the facts observed, specific to each person, produced information. The dialogue between the photographer and the anthropologist at the time the photographs were being chosen on the proof sheets facilitated the confrontation and combination of a visual language and discursive comment. With respect to methodology, the study of practices requires the development of particular modes of data production encouraging the entry of non-declarative information. Depending on the image can contribute to this. Because the photograph maintains contiguity with the referent but also a spatial and temporal distance in relation to it, the distance between the moment photographed and the image contributes to a surprise effect. During research process, the photograph provides a possibility for the researcher to go back and forth between the image and the subject in order to see more and better (Piette 1992:27).¹⁹ Let's take one of the most important photographs, the one presented above: 11. It shows a relationship of a couple which is both normal and unexpected. This image is surprising; it is both familiar to a European observer because it shows codes which enable him to affirm with certainty that this is a married couple, and, at the same time, it is surprising because he does not necessarily expect to discover such a scene in West African society.

The existence of these images requires us to fully consider the dimensions that the written word reduces. At the same time, these images constitute reflexive data; they reflect the interpretations of the anthropologist and the photographer produced on the ground; they are the mark of the intuitions of the photographer (Antoniadis, 2000:141). The photograph, the video are essential tools for the data entry of specific observations and also provide information on the "view cast on things."

The reproduction of research results can be facilitated by use of these media. In order to meet these two objectives (collection and reproduction of data), the researcher should meticulously specify the way in which he or she intends to use these tools. He or she should submit the use of the image to criteria as strict as the other approaches of anthropological research (note-taking, writing, etc.). The photograph (like film) is not a tool which can replace other modes of data production. Written notes compiled in the anthropologist's notebook are indispensable. Writing enables the production of explicative discourse and it is essential to the scientific approach. At the same time, in understanding the world through what is visible, we make the subject of research more complex. Collaboration between the photograph and the written is enhanced if it takes effect both during the research process and during the presentation of results.

Notes

1. This reflection is the result of a collaborative work with Katrin Langewiesche, anthropologist, and Franck Pourcel, photographer, cf. Attané, Langewiesche (2000, 2006), Attané, Langewiesche and Pourcel (2004), Pourcel (2003).
2. The photographs of Claude Lévi-Strauss among the Bororo are a good example of this.
3. Cf. Bateson & Mead (1942). On their work method: Jacknis (1988), Tanio (1994).
4. Cf. Fiéloux, Lombard & Kambou-Ferrand (1993), Ouédraogo (1996), Werner (1993, 1996, 2000).
5. Cf. Blanchard & al. (1995), Blanchard & Boëtsch (2005), Boëtsch & Chevé (2002), Boëtsch & Savarese (1999) ; Ouédraogo (1991).
6. Cf. Achutti (2004), Guran (1996), Lombard & Fiéloux (1998), Piette (1992, 1996, 1998).
7. Jean-Pierre Olivier de Sardan (1982) shows the indisputable supremacy of the written over audio-visual production in the field of production of scientific knowledge, and what he writes about the audio-visual tool can go in large part for photography as well.
8. Cf. Darbon (1994), Gombrich (1983), etc. They are also recalled in various contributions in the special issue of *Journal des anthropologues*, 2000:80-81.
9. In particular with the argument that Geertz (1986) summarizes as, "I was there, thus it is true"
10. This can be summed up by saying that this is the basis of the photographic act includes three main elements: the photographer, the photograph (the image) and the spectator (the person looking at the image). This idea, inspired by Barthes (1980), was picked up again by Antoniadis (2000:121).
11. Cf. Darbon (1994); Guran (1996).
12. This can be summed up by saying that this is the basis of the photographic act includes three main elements: the photographer, the photograph (the image) and the spectator (the person looking at the image). This idea, inspired by Barthes (1980), was picked up again by Antoniadis (2000:121).
13. This potentiality of the image is firmly defended by Albert Piette (1992, 1996, 1998).
14. Thus, the production of the photograph becomes, just like the interview or observer participation, "a privileged place of production of "interpretive models from on the ground" tested as they emerge" (Olivier de Sardan 1995:85).

15. "Jokes which (...) are often used in La Hague in which the irony both hides the fears that one does not want to admit and translates the consciousness of the impotence of men to protect themselves, except in a pathetic way, against this energy" (Zonabend, 1989:33). People talk about enormous lobsters said to be under the cod water discharge pipe of the nuclear power plant in Flamanville, or sheep with five legs, cows that died mysteriously.
16. On this subject, see, Sperber D., *Le savoir des anthropologues [Knowledge of Anthropologists]*, 1982:18-19, to which Piette refers. According to Darbon, the language of the image "needs a text to express its full effectiveness" (1994:116).
17. According to Gombrich, "a correct interpretation depends on three variables: the code, the legend, and the context. One might think that the legend would suffice to make the other two superfluous. But our cultural conventions are too supple for that to be the case" (1983:328).
18. Milton Guran suggests the realization of an effective photograph. (1994; 1996:363 and following). Terrenoire (1985) stresses the fact that the photograph should be scientifically constructed.
19. This is what Albert Piette (1992:27) calls the principle of distance.

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12

Comparison: A Foundational Approach in the Social Sciences

Cécile Vigour

Introduction – A Paradoxal Situation

While comparative research is on the increase at the initiative of national and international organizations, we might be surprised at the relative lack of methodological and epistemological reflection on this approach. Very often, so-called comparative studies consist in a juxtaposition of national monographs, leaving it to the reader to proceed with their comparison. This situation is all the more paradoxical in that the founding fathers of social sciences – whether it be de Tocqueville, Durkheim or Weber – made comparison an essential heuristic approach. In *Rules of the Sociological Method* (1895) Durkheim exclaimed: “comparison is sociology itself.” It would seem important to go back over the principles and the issues of a comparative approach.

This approach seems all the more stimulating in that it is characterized by great diversity: comparisons over time (over several decades or centuries, like Weber working on the formation of the modern State, or Elias analyzing the “civilization of mores”); in space (between countries, federated states, regions, cities), but also between sectors of activity or study of the impact of social and political determinants of certain practices (voting behavior, unemployment, or leisure activity, for example, studied according to the social origin, educational level, profession, etc.).¹

At the same time, we note a change in the issues of comparison. In the 1960s, attention given to this approach was inseparable from the desire to assert the superiority of the western economic and political model, particularly in political science (Almond, Verba 1967), but also in demography and economics (Rostow 1963). In sociology, this revival is associated with the rediscovery of the founding

fathers. Today, globalization (through supra-national organizations like the United Nations, International Monetary Fund, etc.) and the construction of regional entities (whether it be European construction or free-trade zones) are indisputably a driving force behind the development of comparative research. In such a context, comparison is a privileged tool of investigation to analyze transformations of the modern world (Lallement et al. 2003:9).

This article, designed to open the tool box of the specialist in comparison, will be mainly devoted to questions which are raised before comparative research. Before undertaking this, we first need to ask what this approach can bring to the identification of the characteristics of a comparative approach and its issues: (1) Insofar as all research is influenced by social, political and historical factors, it is up to the researcher to identify them in order to distance himself from them; (2) The main concepts and the problem, developed simultaneously and jointly with the first comparisons in the field, are the “compass of the specialist in comparison”; (3) Various comparative strategies can then be implemented – pragmatic or theoretical, determined *a priori* or during the research, according to the observation and analysis scale chosen and methods implemented; (4) Although these comments are so many warnings or methodological preconditions (“tricks of the trade”: Becker 2003), the implementation of a comparative approach always includes a significant amount of tinkering, as in all research, according to the concrete, methodological or theoretical difficulties encountered. We see that reflecting on comparison means going back more generally to the approach of knowledge production in the social sciences and to some major questioning: the tension between the general and particular, between indexation on context and abstraction; the link between the micro, meso, and macro levels, etc.

Comparison: To What End?

Although this question is indispensable, it seems to be too rarely raised. However, beyond the effect of mode, we should think about the relevance and importance of conducting a comparative study, by wondering what a comparison adds that research on a single field would not allow us to observe. Furthermore, comparison is often envisaged starting from the formulation of the project, even before beginning research. Yet, the question of the appropriateness of implementation of a comparison is also often raised during research. Indeed, a researcher can research in a field, and develop hypotheses, then ask if they are relevant for only the case studied or if the explanatory model drawn up is more broadly valid, and then work in other fields to test its relevance. It is always possible, even after having completed the basics on the ground, to analyze data collected to examine it comparatively.

What is comparison?

Giovanni Sartori (1994:22) starts from a simple example to define comparison. He asks to what extent apples and oranges are comparable. In his mind, judging the relevance of such a comparison requires asking, “comparable with respect to which properties or characteristics and incomparable (for example because of too many differences) compared to which other properties or characteristics.” He concludes in a first analysis: “The important thing is to remember that comparing is both assimilating and differentiating with respect to a criterion.” Thus comparing is, in a first approach, to highlight differences and common points according to a criterion which should be defined from the beginning and which directs the attention of the researcher. This operation is then not part of the evidence, but is the subject of a construction.

Moreover, comparison is not only a technique or a methodology;² indeed, the specialist in comparison is likely to mobilize a great diversity of qualitative and/or quantitative methods. Comparison is more broadly a research strategy which permeates the whole approach of the researcher, from the definition of the problem, to the choice of field, the construction of data, their analysis and explanation. Rather than a set of tools, this is an intellectual stance,³ which encourages us to move away from the center and which has no disciplinary boundaries.⁴ This “detour of knowledge” (Lallement 2003: 107) is a “systematic comparison of modes of construction and evolution of the same social fact between national spaces,” but also over time, between sectors, etc.

Systematic comparison in the social sciences, concerns three main dimensions: the actors, institutions and categories mobilized. For each of them, the synchronic (at a given moment) and diachronic (inscription in the long period of their constitution) dimensions should be considered in such a way as to highlight the effects of sedimentation and historical dynamics. Thus understood, comparison invites us to put processes and trajectories in perspective. Moreover, the specialist in comparison should not consider isolated elements, but pay attention to the way different parameters that he identifies as characteristics of the social fact studied in a given context lay themselves out specifically. He shows configurations and the way in which they are transformed. Finally, insofar as the comparison is a construction of the researcher, this operation requires a reflexive attitude, namely the ability to step back to reflect on conditions of categorizations that he is creating and procedures of analysis that he is implementing. Attention to process and to configurations and reflexivity on his position are the key words of the specialist in comparison.

Issues of the comparison

This suggestion of creating some space with respect to what the researcher knows is precisely one of the first issues and contributions of the comparison (see Table 1). In a system that we can qualify as epistemological, in the comparison with societies where the fact studied appears differently, it is important to break with prejudices and preconceptions (Durkheim 1981 [1896]) or with ethnocentrism (Boas 1968 [1896]), in becoming better aware of the status of the social, political and historical construction of this fact. Comparing also enables us to better understand. This can be a descriptive objective pursued in itself or with the concern for “learning lessons from it” – like de Tocqueville studying American democracy with the objective of better understanding the political transformations taking place in France (1986 [1850]).

Table 1 – Objectives and Systems of Comparison

Objectives	System of Comparison	Issues
Distancing	Epistemological	Break with ethnocentrism.
Better understand	Descriptive	An objective in itself or the desire to “learn lessons”.
Classify, order	Explanatory	Suggest a typology of facts observed
Generalize	Theoretical	Find social regularities and main factors. Produce an explanatory model.

Classifying and ordering can be the third objective of a comparison. In this case, the researcher often attempts to produce a typology of observed facts. This effort at clarification of thought by sequencing of reality is often a precondition for explanation. But it is not pas always devoid of value judgments, especially when the classifications realized are part of a practical and political objective of administration or when they go along with the implementation of public policy. The debates on typologies of welfare states proposed by G. Esping-Andersen (1999 [1990]) – considered as promoting the Scandinavian model and not sufficiently taking into account the question of gender (Jenson 1997) – are there as a reminder. Finally, comparison is often aimed at generalization. In this case, it is a matter of finding social regularities as well as the main factors which influence the social fact. The issue is to produce an explanatory model. Comparison can then be mobilized as a substitute for experimentation (this was the great hope of the founding fathers) and as tool to test hypotheses.

For M. Dogan and D. Pélassy (1982:185), typology and model are both a “synthetic conclusion capable of inventorying, clarifying and systematizing the results

of the comparison.” But they first differ in their unequal explanatory power: typology organizes the universe, based on a rather descriptive analysis, whereas the model explains reality based on an analysis of the causal type. Furthermore, these two tools differ one from the other by their more or less great ability to analyze change: while the former corresponds to a static perspective, the second is part of a dynamic approach. “The model includes movement; it is not pure chance that contemporary research is more directed towards this pattern in action than towards too rigid actions [...] whereas typology tends to freeze the reality what it wants to synthesize, the model attempts to perceive the way in which the process unfolds over time. Typology compares several stages of social and political development. The model films the change itself, but not without this quest raising some problems” (Dogan and Pélassy 188). Finally, whereas typology tends to respond to a concern for exhaustivity,⁵ the model is more characterized by its selectivity. These two types of formalization of results fulfill different requirements results, all the while showing a concern for rigor.

Identifying the Social and Political Issues to Distance Oneself from Them

At the same time, the specialist in comparison cannot neglect reflecting on social and political factors which influence his work. As Michel de Certeau stresses (1974:21), “a subject has a history and all research [...] is based on a place of socio-economic, political and cultural production. Research involves an environment of development that is defined by tidy determinations [...]. It is subject to constraints, linked to privileges rooted in a particularity. It is according to this place that methods are established, that a topography of interests is defined, and that matters and questions to ask of documents are organized.” The researcher is called on to exhibit critical vigilance: “all human science should introduce the suspicion of its own development to question its historical relationship to a social type. It goes hand in glove with a form de culture. In order to redefine itself, it should proceed with a dissenting analysis of the civilization that it posits. Between a society and its scientific models, between a historical situation and the intellectual baggage which is suitable for it, a relationship exists which is a cultural system” (de Certeau 1980:166-167). This caution comes in four ways: in questioning the relationship of the researcher to his subject; in distinguishing systems of expertise, research and social debates social; being aware of the political stakes of the comparison and avoiding the introduction of cultural biases.

Questioning the researcher’s “relationship to values”

Weber was already urging this call for vigilance in “The Objectivity of Knowledge,” by encouraging the researcher to wonder about his “relationship to values.” This relationship to values is perceptible in the choice of subject, the way of understanding it or the selection of factors judged to be important – so many

more or less conscious preconceptions linked particularly to the origin (social, ethnic, national, etc.) of the researcher, his education, his sensitivity and his period. This is what would explain that faced with the impossibility of rendering an account of a fact in its global nature and all of its complexity; a single phenomenon can be analyzed in a very different way, depending on the point of view adopted. These unavoidable choices are not neutral. This is why the specialist in comparison should become aware of their subjectivity (which is not arbitrary for all that, if it is justified).

This reflexivity does not mean to lose one's values, but to be aware of them to overcome as much as possible the impact that they can have on the formulation of problems, the direction of the viewpoint and the conclusions of research. Thus, the researcher should always identify the various issues: scientific, political or those on the agenda for public debate. It is also useful and necessary to objectivize his own social position with respect to research subjects or to possible backers. This "double work of clarifying prejudices and of objectivization of [his] position" is called "auto-analysis" by Florence Weber and Stéphane Beaud (1997:26). These authors recommend, before even beginning the research and all throughout, to think about exact, concrete factors having encouraged the researcher to choose his subject rather than another, often linked to his personal career, his education, etc, but closely dependant on the intellectual, social or economic context which he is in. Likewise, they also recommend thinking about themes which were first excluded.

It is for this reason, according to B. Jobert (2003) that it is impossible to claim that comparison in the social sciences has the same status as experimentation in the natural sciences, because it greatly depends on the observer, his value system, theories that he uses, the scale where he places himself, the means that he adopts.

Distinguishing between systems of expertise and those of research

Numerous social and political determinations influence this work. Comparison is indeed "a reality constructed institutionally, politically and scientifically" (Commaillie 2000:111). This is why the researcher should attempt to establish a distinction between the expertise forum, the knowledge forum and the media-related forum (see Table 2).

Table 2 – Types of Forums and Issues

Types of fora	Issues	Objectives
Research forum	Scientific	Objective of knowledge
Expertise forum	Political, sometimes ideological	Concern with action
Social forum	Social and media-related	Mobilization in debates or controversies

J. Commaille (2000) suggests that we distinguish two dimensions. In the research forum, research would be conducted with the objective of knowledge, while the expertise forum would be characterized by a concern for action according to which the comparison “would consist principally in attempting to find convergences and divergences.” In this latter case, the study is explicitly directed according to a demand, whether it be from a national public institution (a ministry, for example) or an international institution (World Bank, Organization for Economic Cooperation and Development, etc.). According to J. Commaille, there is a growing confusion between the research and expertise fora, particularly in the framework of research encouraged by the European Union or other international organizations. This may contribute to a blurring of the status of comparison. Thus, a number of European studies are based on the *a priori* hypothesis of a convergence between European countries without, however, showing it.

In a yet more critical way, F. Schultheis (1990:227) stresses particularly the influence of “the social demand” on comparative studies, stigmatizing “the specificities of the social use of the currently dominant intercultural comparatism: produced by scientific specialists (statisticians, economists, etc.) based on a public demand from an international institution, endowed with all the symbolic qualities of both scientific and political credibility and made ‘official’ before becoming ‘public’, the information given becomes an issue in political discourse, capable of fulfilling the strategic functions attributed to it.” The danger of such comparisons lies in the public issue that they constitute, particularly ideologically speaking. The comparative approach of the expert is directed – and sometimes biased – by the concerns coming from an institution, which already provides a problem which is not necessarily the most relevant.

But this orientation towards practical objectives can also be seen in research which is not explicitly ordered by international institutions, but which is aimed explicitly at identifying best practices (“*benchmarking*” method; Bruno 2007). Although it is perfectly legitimate to consider that research in the social sciences can contribute to improving knowledge on contemporary societies and better take into account the challenges with which they are confronted, at the same time, the

concrete study protocol will be different depending on whether it aims to understand what is or tries to determine what should be. The risk is then great to stick to the single normative dimension. This is what we could qualify as “technicist biases.”

Moreover, we should add a “social forum” to these two fora which would correspond to social and media debates in which research (specifically quantitative), is mobilized, often in a polemic fashion. There is no dearth of examples: the evolution of the academic level, the question of social mobility, or the pauperization of a part of the society. Yet, whether their authors want it or not, work in social sciences on these themes or the data that serve as bases for them will be used as part of these controversies. Researchers who work on these subjects cannot forget it. A particular status is given to comparative research results, used in their most simplified formulation by the media, without considering the limits that the researchers themselves ascribe to their study and their analyses (Blum and Guérin-Pace 1999).

Finally, it is important to stress that these fora are not airtight, that their boundaries are blurred, and all the more so in that some actors (researchers, political actors) speak in these various fora – either at the same time, or throughout their careers. For example, some researchers who work on schools are approached to participate in various national or European decision-making bodies with expertise on educational reforms. They are also regularly called on to speak in the media, to explain the stakes involved in such or such program change, pedagogy, teaching methods.⁶

Being aware of the political stakes of comparison

Among the stakes in which comparative research is inserted, some stress their particularly political or even geo-political dimension. In the context of globalization and Europeanization, we indeed observe a boom in borrowing and transfer practices.⁷ “The process of production of frames of reference is becoming internationalized; states’ fight to impose their own vision of the world is becoming more crucial. The development of international comparisons is a part of this search for hegemony” (Jobert 2003:325).

As a result, the specialist in comparison should be twice as careful, particularly at the start of his work. He should identify the demands of the institution which is placing the order, and more generally understand the social and political stakes associated with the theme addressed, reformulate the subject of research by distinguishing between questions of expertise (the implicit or explicit social demand) from research questions by reconstructing the research subject based on scientific concepts and questions; and finally, giving the framework and specifying the limits of generalizations and conclusions which can be drawn from them. The controversy in the French academy on the new ministry of immigration and national identity are

illustrative of the influence of such socio-political stakes. This debate particularly revolves around appropriateness of accepting financing from this Ministry. To what extent does doing so run the risk of influencing questioning and research directions on immigration, but also encouraging the political appropriation of research which will have been conducted on this theme? Others consider that it is all the more dangerous to only give the floor to researchers who are politically close to power. Finally, others campaign in favor of the development of research on these themes without financing from this ministry. The debate is on in France. But all researchers are led to question the possible influence of the backer and his (implicit or explicit) expectations on research results. Despite all of this, there are numerous successful examples of sociological recasting.⁸

Be vigilant vis-à-vis the risk of introducing ethnocentric biases

Finally, another pitfall often threatens international comparisons, that of the ethnocentric approach. This risk requires that the specialist in comparison be extremely vigilant with respect to what goes without saying and to distance himself from what is familiar – in such a way as to avoid introducing cultural biases (...) In another form, we find the Durkheimian warning with respect to preconceptions (Durkheim 1986 [1895]).

The first form of ethnocentrism is found in a formulation of the problem too rooted in a particular culture: “when the intercultural comparison consists in studying other cultures, a question which has arisen in the context of a particular culture, it is rare that it proves to be really intercultural. This sort of project leads to a style of cooperation for which Adam Schaff used the aggressive expression “colonialism of research” (Scheuch 1967, cited by Dogan and Pélassy 1980: 32).

This tendency is seen particularly in international research designed by generalization of a study first conducted in a single country, then extended to others, without reworking the initial framework of analysis, questions, and methodological framework. The risk is again great when the realization of the study is delegated to national teams (who are principally involved in translating the questionnaires, their editing, and data transmission). Although the concern for obtaining comparable and homogeneous results is legitimate, it does not always translate into the development of relevant procedures, in encouraging the building of fairly strict and similar research protocol in all countries studied (with respect to the way of writing questionnaires, coding responses). The risk is then that the results do not assume a real relevance.

An international investigation into illiteracy provides a good example of this. This assessment conducted in the 1990s is based on the comparison of results of tests designed with the help of an indicator of “literacy”, created to designate abilities of reading and comprehension of texts in daily life (classified into four levels of difficulty). The results, immediately contested by the French Ministry

of National Education, indicated that “three quarters of French people have a level of ability in terms of literacy which does not allow them to conduct activities in daily life: read a newspaper, write a letter (...)” (Blum and Guérin-Pace 1999:274). Alain Blum and France Guérin-Pace identify two main sources of errors: different levels of difficulty for a single question following the translation (whereas the hypothesis of the universality of the scale of difficulty of tests is posited) and the existence of geographical and linguistic differences. Indeed, they show (with the help of an ascending hierarchical classification) that test results are closely correlated with the linguistic or cultural proximity with the United States and Canada, and more generally with Anglo-Saxon countries. The authors show that cultural biases call into question the very design of the research, inspired by the American experiment, the *National Adult Literacy Survey*: “The main criticism which can be directed at the research (...) is precisely to be based on the idea that there is a single model of society.” As a result, the implementation of multinational teams during the development of a research framework and during the research⁹ appears very successful in reducing the existence of such biases as much as possible.

As a result, the ideal position of the specialist in comparison (as for all researchers), is probably more similar to that of the foreigner, skillfully described by Simmel (1999 [1908]). According to him, the foreigner is characterized by the tension between proximity and distance, between empathy and freedom of judgment, critical distance – so many qualities to cultivate. Furthermore, we should note that although the risk of an ethnocentric approach can rightly be understood as one of the major pitfalls in the comparative approach, it is also one of the benefits of comparison to allow this distancing with what seems obvious to us and encourages reflexivity.

Concepts and Indicators: The Danger of Term-for-term Comparisons

In comparative research also, concepts and problem make up the centerpiece of research. Their definition constitutes “the compass of the specialist in comparison” (Dogan and Pélassy 1982). This is what will next allow us to possibly develop ideal types and to choose relevant statistical indicators.

Concepts, the true “compass of the specialist in comparison”

Main concepts provide meaning for the comparison. Insofar as the social facts are particular social, historical and political constructions, peculiar to each country (indeed to each region, professional group, etc.), the phenomenon studied should be the subject of a work of (de) construction. This means avoiding term-for-term comparisons and those which are based only on terminology. The same word can indeed cover distinct realities. This difficulty is increased when the languages of the countries studied differ. Thus, it is sometimes difficult to find an equivalent for the French term “cadre” (in Germany, for example). This reminds us that the

category of “cadre” is indeed the result of a socially situated historical construction (Boltanski 1982). It is important to deconstruct categories in order to see what they are concealing¹⁰: this is in itself a major stake of comparison. In this sense, comparative analysis plays another role, because it enables the researcher to avoid naturalism. Indeed, the fact of observing that “elsewhere, things are different” is a rampart against the illusion that the phenomena which surround us are “natural.” Comparison then allows us to surpass the evidence, the “it goes without saying.” Because, as E. Durkheim stressed (1975:147), “doing comparative sociology is not simply pulling together a bit hastily all sorts of materials: it means first providing the critique of them, and then submitting them to a development as methodical as possible.”

A rigorous definition allows us to compare *a priori* very distinct subjects. Thus, for example, what is comparable at first glance between the magistrate, the elementary school teacher and the nurse? This would seem to be a “comparison of the incomparable.” In his work on *The Decline of the Institution*, F. Dubet (2002) shows that these different professionals, but also social workers, mediators, high school teachers and trainers for adults, participate in what he calls “work on others,” defined as “the set of professional activities participating in the socialization of individuals.” These professions are based on an “institutional program,” characterized in France by its exteriority (they refer to values which are defined as outside of the world, in the way of an opposition between sacred and secular levels), the dynamic of the vocation (which resistance to professionalization renders an account of) and the tension at the core of socialization meant to construct a subject which is both socialized and autonomous. This is the definition of this concept which creates the relevance of such a comparison and its ability to analyze in the most general way the current transformations of institutions. Likewise, the concept of repertory of collective action, mobilized by C. Tilly (1986:541) to designate “ways of acting in common on the basis of shared interests,” allows us to compare both in space and time phenomena which are, *a priori*, very different – from peasant uprisings of the 17th century to anti-globalization protests to strikes and election campaigns.

The degree of specificity of concepts differs depending on the extent of the comparison. This is what G. Sartori (1994) designates under the term scale of abstraction.¹¹ The principle is the following: the more a concept is indexed on a particular case or field, the more it is defined by specific characteristics; the more the researcher wants, on the contrary, to extend it to other case, the more the concept loses its specificity (and the less the definition of the concept will include specific characteristics). Although they do not use such an image, Barney Glaser and Anselm Strauss (1967) also stress the necessary despecification of a concept to describe and compare a larger set of facts. They take the example of the end of life as an “unpredicted change in status.” This conceptual category includes the

following properties to characterize the status of the dying: it is an almost inevitable transitory situation (except for sudden death) which can occur at different ages and stretch out over a duration which is also variable and which, in western countries, is more and more often taken care of in medical institutions. If the sociologist is only interested in terminal cancer patients, he can characterize this situation in an even more specific way. If, on the contrary, he wants to extend the field of analysis to all situations of passage between two statuses, such as “youth,” engagement, the fact of being a prisoner, etc, he will qualify this concept in a more general way. It is up to the specialist in comparison to define his position on such a scale of abstraction, according to his objectives and the number of cases studied.

The definition of the exact research subject and the development of a problem – essential steps – thus go hand in hand. Moreover, there is always a back-and-forth between the theoretical construction of the subject and the comparison with the field or fields. To render an account of this continual dynamic, D. Cefaï (2003) speaks of a “spiral of research.”

Ideal types: stylization tool of the social fact

From this perspective, we can understand the importance that Weber accords to ideal types which are designed to clarify the main characteristics of a phenomenon and its variants according to cultural, historical and social contexts. The ideal type – “the non-contradictory cosmos of thought relations” (Weber 1965: 179-180) – gives a synthetic résumé, without claiming to include or render an account of the social reality in its totality. For Weber, it is neither an issue of a description of reality, nor of a hypothesis, but of a selection of particularly characteristic traits of the fact studied. The sociologist stresses empirical variations and the relative character of example defined which are often combined in reality. “We obtain an ideal type by accentuating unilaterally one or several view points and by threading together a multitude of phenomena given separately, diffused, and discreet that we sometimes find in small numbers and at times not at all, that we order according to the preceding points of view chosen unilaterally to form a tableau of homogeneous thought” (Weber 1965:196). This “analytic construction” is a fruitful heuristic instrument for the specialist in comparison.

Thus in his work, each social configuration (Protestant or Confucian ethic, market economy, etc.) or process of civilization (modes of political or religious legitimization, and the passage from one to another) are stylized by some of their main characteristics. By the comparison between two ideal types or the comparison of an ideal type to the historical reality, the objective is to establish relations of causality or “elective affinities” between certain parameters thereby located, as well as tendencies towards change. In the “Introduction” to *Economic Ethics and World Religions* (1996 [1915]) for example, Weber identifies three types of domi-

nation, depending on the foundation of their legitimacy. Traditional domination is founded on respect for what has always been (actually or supposedly); ancestral traditions are the guarantor of the legitimacy of rules and authority. Legal-rational domination is based on conformity to the rules of law. The modern bureaucratic state is the purest example of this type of domination; an impersonal norm defines the competence of the civil servant and the extent of his power, characterized, moreover, by the separation between public and private spheres. Finally, in the case of charismatic domination, individuals submit to orders or rules stated by a chief, by virtue of the sacred or exemplary character with which they invest him, whether he be a religious prophet or political leader. Domination is not exerted according to general norms or those from tradition; it is revolutionary.

The definition of these three ideal types allows Weber to more precisely analyze concrete governments (knowing that reality often corresponds to a combination of these types of domination) and especially to describe the passages from one type of domination to another. In the past, change in domination was often achieved by alternation between traditional and charismatic authorities. Weber shows that these are the processes of material rationalization (of administration and justice “by a prince who satisfies his subjects from the practical standpoint and that of social ethics”, p. 374) and formal rationalization (domination by legal norms) which have enabled the accession of a legal type of domination and bureaucratic domination. Bureaucracy is characterized by a particular type of man, professional lawyers, to whom a role and duty are attributed, and whose competence is defined by impersonal norms established rationally.

Statistical indicators and categories: an often problematic form of comparability

Tables of computed data are probably the most obvious form of comparability (and potentially the most “dangerous” since they are seemingly objectified in the form of statistical categories); this is why the statistical comparison requires the greatest caution and rigor. The statistical monitoring approach should include both a critique of the quality of indicators (with respect to their construction and reliability) and a critique of the use of indicators, a concern of for contextualization and attention to effects of temporal discrepancies. The specialist in comparison should take into consideration the context in which the phenomenon studied is inserted, as well as the context of data production.

On the one hand, statistics sometimes conceal different realities. For example, Eurostat data on family benefits exclude in the case of France certain “fiscal benefits” (reductions in taxes ensured by the family quotient) and “housing benefits” which, however, contribute to the well-being of families. The comparison of unemployment rates (Maruani 1996) is also a good illustration of this. Not only are there three international definitions of unemployment, but different contin-

gent parameters (such as modalities of indemnification of men and women) influence the rate of registering as unemployed. If married unemployed people are clearly more numerous to receive indemnification than single unemployed people, and this is true in all countries, on the other hand, the situation is more complex for women. Systems of indemnification unfavorable to married women in Great Britain, Ireland, the Netherlands and Germany contribute to classify some of them outside of the workforce, as their unemployment gradually continues over time. From then on, only the comparison of percentages of unemployed individuals, without taking into account factors which influence their definition, can lead to erroneous interpretations. The researcher should then reflect on the modalities of construction of statistical categories and more generally on “the share of the social norm which, beyond rules for indemnification and enrolment in the unemployment system, pushes women to call themselves unemployed or define themselves as inactive”. As a result, the qualifier of unemployment or outside the workforce is not only a question of statistical counting but has a sociological foundation. These examples stress the danger of synthetic comparisons denounced by F. Schultheis (1989).

On the other hand, “everything else being equal” comparisons are not without their problems. The “everything else being equal” hypothesis is particularly the foundation of the analysis of regression. We then construct a model without interaction.¹² François Simiand (1903) already indicated about economic comparisons, that statistical requirements come back “to wondering how would a camel live if, remaining a camel, he were transported to the polar region, and how reindeer, if they remained reindeer, would live if they were transported to the Sahara.” This is why categories and statistical relations should be made explicit compared to the social and historical context.

The researcher should also reflect on the mode of construction of “samples” on which he works in order to beware of the “trompe-l’oeil of a sample,” to use Jean-Claude Passeron’s example (1991:123): “most populations on which measures operate and in which we take a “representative sample” are already prefabricated samples by a social process.” Indeed, these populations are the result of a social and institutional action; for example, populations of university students are the result of the selection made by academic institutions which precede the university. Questioning on the mode of construction of the sample also concerns the groups which are seemingly “natural” (such as groups of readers, etc.). The objective is to limit the dangers of “rampant induction” in interpretations concerning the influence of sex, age, class, ethnicity, etc on the propensity for culture, illness, crime. The researcher should be particularly attentive to the phenomenon of categorization by institutions, whether it be school, hospital, or police (Aubusson de Carvalay 2002; Mucchielli 2002). These problems refer to the research method (collection and construction of data, development of research protocol, etc.) as well as the rigor of the interpretation of figures and categories.

“Social demand” also has an impact on the data collected through the choice of the definition of the subject and the socio-political context in which the research is conducted. As A. Desrosières (1996) stresses, the fact that a question – such as the mistreatment of a child – becomes socially and politically sensitive (i.e. the fact that it is a public action), transforms its statistical status. “Tracking procedures (toll-free numbers), recording and counting are put in place. Definitions and criteria are formulated. When this operation is still recent, interpreters hesitate between two readings: the number of mistreated children has increased or observation procedures have improved. We already observed this hesitation with respect to unemployment in the 1960s, with the progressive implementation of the National Agency for Employment (ANPE, in its French acronym). This makes commentators, who cannot convince themselves to renounce realistic rhetoric and criticize the uncertainties of the system of observation which cannot provide them with reliable figures [...] ill at ease. Reliability is closely associated with stability and the routinization of the chain of recording and counting, which implies that the subject has become less of a burning issue.” Thus, as the socio-political context and media-related topicality have an impact on the statement and collection of data, interpretation over time of evolutions of certain facts should be carefully conducted.

By drawing attention to the dangers of certain statistical comparisons, we are not, of course, casting doubt on analyses of this type, but stressing the necessary vigilance of the researcher and the methodological precautions to take. Comparison allows us to relativize categories of analysis, which are often assumed to be natural, whereas they are social constructs (Maurice Sellier and Sylvestre 1982:107-108).

Some Comparative Strategies

At this stage of research at the latest, two main questions arise for the specialist in comparison: which cases to compare and how many? According to which criteria are these choices made? We present below some of the modalities of selection, without any claim to providing an exhaustive list.

An often pragmatic choice

It is most often a pragmatic choice which prevails in the selection of such or such country. Here are some of the criteria which, consciously or not, have an influence on the choice. The first is without a doubt the country of which belongs.¹³ Institutional factors play a crucial role, among which we could include the sources and conditions of financing, whether it be special partnerships with certain countries (for example, in France, much comparative research includes Germany, a fact that we can relate to the existence of institutional incentives and financial systems, like the Interdisciplinary Center for Study and Research on Germany (CIERA) or European financing (in this case we find conditions with respect to

number and geographical diversity and in the size of countries to be included in the comparisons). Knowledge of the language and affinities with certain cultures, or access to research fields (geographical distance, financial cost) influence the selection of units. Moreover, most comparisons are marked by the national framework (despite the context of reconsideration of the state from the top – deployment of supranational organizations – or from the bottom, with regional development and municipal structures.

There is no doubt that numerous practical considerations come into play in the choice of countries, even if theoretical justifications are then advanced to justify it. It is merely important to take care that this choice does not constrain the definition of the subject too much – at the risk of later hindering the research.

Two main ways of choosing countries to compare

Among the multiplicity of types of comparison, two main modalities of selection can be distinguished: depending on their degree of proximity of point of view with the issue or the method of “theoretical sampling.”

Choosing the units to compare depending on their degree of proximity

A. Przeworski and H. Teune (1970) distinguish two strategies of comparison: between “the most similar systems” and between “the most different systems.” In the most similar systems strategy, the researcher studies units as similar as possible, except on the factor studied. Similarities must be ignored to explain the differences. Inter-systemic differences are considered as explanatory variables. The Weberian analyses on Protestantism, comparing several strains of Protestantism and their effects on the relationship to the world and the implication the economic universe are a good example.

In the strategy of the most different systems, the only thing that is important is the common points between the contrasted countries. Thus is the case in Weber’s analyses on China and the west, on Protestantism and Confucianism: Weber attempts to understand the particularity of the development of the state and capitalism such as we have only seen in Europe, whereas conditions were also ripe in China which could have contributed to the emergency of a modern capitalist economy. A subtle comparison of Protestant and Confucian ethics enables him to locate favorable factors and those which have created an obstacle. Skocpol’s book (1985 [1979]), which analyzes the revolutions which occurred in France, Russia, and China, illustrates perfectly the system of comparison between “very different countries.” The author wonders about factors which explain that beyond national, cultural and chronological differences, revolutions took place in these countries. She offers the hypothesis that these three revolutions go back to the same explanatory mechanism: the monarchical state founded on an agrarian society, crisis between the state and the dominant class, the state confronted with an

international crisis. The literature grants the methodological and epistemological primacy to this type of comparison (see also Détienné 2000) – probably because it requires an increased effort at abstraction and analysis, and even if it is not the one which is the more widespread.

It is also possible to combine these two strategies, for example in choosing three countries, two resulting from a comparison between similar cases, two between different cases. This is what David Laitin (1999) did in the framework of a study of nationalist movements. His objective is to understand why some of them had recourse to violence. The researcher first makes an initial comparison, within the same national entity, between Catalonia and the Basque. By highlighting the factors which distinguish the two regions and nationalist movements, he constructs a model of micro-analysis which he then tests in transposing his theory to differences between Ukraine and Georgia. The combination of these two strategies allows him to strengthen the force of the theorization. After showing the insufficiency of macro-theoretical explanations (historical, in particular) to understand the recourse to violence, or lack thereof, the author identifies a group of favorable “micro” factors: a dense rural social structure as a necessary condition; a phenomenon of tipping game (return to the use of the regional language, for example, as incentive factor) ; random events, such as visible victories without the costs really being so (as the exit costs of the nationalist organization and the culture of violence).

We should note that here we have a choice which is made from the outset of the research; the categorization as “similar” or “different” is made based on ideal types. It is precisely this *a priori* of the comparison that other researchers dispute.

Choosing the cases to compare based on the theoretical sampling method

B. Glaser and A. Strauss (1967) recommend the choice and systematic analysis of several groups of comparison, according to a method that they call “theoretical sampling.” For them, the comparative method is a privileged way of producing a theory and it is this objective which should guide the choice of cases. Various stages are set out. Research begins by the in-depth study of a field and the development of relevant categories of analysis. A first level of comparison is developed by the systematic comparison of facts observed in the same place. Then, to specify categories developed from this first field, the researcher is invited to research systematically the “negative cases,” i.e. situations presenting opposite characteristics. If a comparative approach is envisaged from the beginning, on the other hand, the choice of situations to compare is only made during the study, after data collection on a case, according to the needs of theorization and not *a priori*.

Studying the dying, after observing a first department, B. Glaser and A. Strauss (1965) compare the end of life in a newborn department and in a geriatric department (with respect to criteria of age and of the ordinary or unordinary

character of the end of life). Progressively, this researchers extended the comparison to a group of varied situations : various departments in a same hospital (pediatric and geriatric, oncology and emergency, with respect to the duration of the process, etc), different types of personnel in the same department (nurses, doctors, hospital porters, housekeepers, etc.), in public and private hospitals, the end of life in a hospital setting or in another context, different regions of a same country, the United States (where the seriousness of his state is normally hidden from the patient) and Japan (where the patient is made aware of his situation), etc.

By increasing the number and diversity of cases, phenomena can be noticed that were not spotted in the more familiar cases first studied. For example, the role of families had not been systematically studied in American hospitals. Families are unequally approached by medical personnel, depending on the location of hospitals (in the city or country), regions, countries, etc. It is this dynamic process of continual back-and-forth between field work and theory which enables us to develop a scientific theory. There is both production of a local theory (i.e. empirically grounded in the field) and the analysis of data in such a way as to increase generality.

Among other possibilities, the choice of a test country

Yves Surel (2000) conducted a study on European central banks, to see to what extent national styles of public policy are affected by European integration and to assess if we can speak of a homogenization of practices and policies, or if significant national specificities remain. To this end, he chose to compare three countries, characterized by a statute providing little protection for the independence of this institution (contrary to the prescriptions of the Treaty), but which set themselves apart by their membership, or lack thereof, in the euro zone (central banks in France and Italy which should conform to requirements in the Maastricht Treaty, and that in Great Britain). By the presence or lack thereof of an obligation to change the statute of central banks, the author tries to discern the particular effects of European integration and of a “trendy” idea, that is, to give more independence to monetary institutions. Furthermore, a limited comparison is made with Germany, given that the *Bundesbank* is an explicit reference of the reforms. The choice of a test country can enable us to distinguish between economic and structural factors.

The question of number

The question remains as to the number of cases to compare. Any comparison implies the comparison of at least two units. As a result, research on a single country; even if it is foreign, does not constitute a comparison – except if, in a systematic way, parallels are drawn with other cases (as Tocqueville did in *On Democracy in America*). For all that, even if a researcher does not wish to conduct

comparative research (for lack of time, for example), he can possibly compare his results with those of other studies conducted in different countries or sectors. It is rare for a researcher to be able to work alone on more than three or four countries, where there is significant field work. Beyond that number, we often see research collectives (for example, in the framework of European programs), or long-term research (over an entire career) or, of course, statistical studies.

Generally speaking, increasing the number of cases allows us to strengthen the representativity of research, all the while increasing the level of generalization, but as a result reduces the specificity of the statement. The result is that the specialist in comparisons should decide between specificity and generality, both with respect to delimiting the subject, development of concepts, and the choice of the number of cases. Potentially, the more the research subject is limited, the greater the number of concepts mobilized and intervening variables also restricted, the more detailed the comparison can be and the more conducted in a controlled way (Dogan and Pélassy 1982); but it often remains very indexed on the cases. The greater the number of cases, the more global and general the analysis can become. The borderline case is that of exhaustivity.

M. Dogan and D. Pélassy (1982) then propose a typology of modalities of comparison based on the number of countries and their degree of proximity. The authors point out four possibilities, according to the research is based on a “type-case” (deviant case or borderline case compared to a model notably), a binary comparison, and comparison between analogous countries (or systems) or on the contrary the comparison between contrasted countries (or systems). Certain common objectives can be pursued in all of these situations: to posit hypotheses and establish laws (in the sense of finding social and political regularities, and explanatory factors). Nevertheless, the choice concerning the number of cases and the comparative system have consequences both on the method of analysis, conceptual instruments (number, degree of specificity) and the degree of abstraction of the conclusions which are drawn (See Table 3).

The Choice of Mode of Comparison

The comparison scale

The choice of comparison scale depends not only on the scope or region of the research subject, but also the way in which it is studied.¹⁴

It would seem necessary to find the proper observation distance. Simmel (1981 [1917]), with the concept of “variable distance,” underlines the fact that, depending on the distance chosen by the observer, he or she will understand the same object of observation in different ways: “When we see a spatial object at two, five, ten meters, each time we have another image, which is only accurate each time in each particular case and which can give rise to errors within these

limits. If, for example, we consider a painting from a few meters away, whereas we have previously looked at the detail in one particular part, the new vision will be totally deformed and distorted, although based on superficial concepts, we can state that the detailed view is more true than the painting seen from afar.” The viewpoint of the researcher differs according to the level of analysis where he is located. He does not perceive reality in the same way, depending on whether he observes with the naked eye, with glasses, with binoculars or a microscope. The researcher, in choosing the comparison scale, the proximity or the distance, is led to take a concrete stance in the debate over the link between the micro, meso and macro levels.

This idea that the level of analysis chosen does not only influence the degree of specificity in the study of historical and social facts, but what we observe is at the heart of reflections on the work of micro-history.¹⁵ First main hypothesis: when the researcher modifies the scale at which he positions himself to analyze the social, it is not only the focus of observation which changes, but the very nature of what he is observing: “the choice of a particular scale of observation produces effects of knowledge [...] Varying the zoom lense of the objective, is not only to increase (or decrease) the size of the object in the viewfinder, it is to modify its form and framework [...] Playing with the scales of representation in cartography does not mean representing a constant reality in larger or smaller size, but to transform the content of the representation (i.e. the choice of what is representable)” (Revel JXE:19).¹⁶

Secondly, “the principle of the variation of the scale [is] an exceptionally fruitful resource because it makes possible the construction of complex subjects and taking into the account the shatterproof structure of the social” (Revel JXE:13).¹⁷ This other “cartography of the social” allows us to render an account of the complexity of the social facts, in order to “better understand the entanglement of social systems, also better resisting the temptation of reification of actions and relationships as well as categories which allow us to think about them” (Revel JXE:13). Social practices are put in perspective and replaced in particular social configurations in which they take a place: it is possible to understand the multiplicity of spaces and times, as well as the web of relations in which the fate of a man or a group of men is registered.

Table 3 – Four strategies according to number of cases and system adopted

Definition and examples	Case Study	Binary Analysis	Analogous countries	Contrasted Countries
	Monograph, particularly deviant and borderline cases.	Comparison of two units.	Similar countries which differ with respect to phenomenon studied.	1. countries presenting a maximum amount of contrasts and 2. relevant contrasts (by the choice of exemplary countries).
Objectives		1. Put forward hypotheses. 2. Establish laws.		
When to use it?	1. To test a hypothesis. 2. Specify a problem.	1. Refine knowledge of certain processes. 2. Stress specificities of cases. 3. Thus set an intermediate level of generalization.	To neutralize certain differences to better analyze others.	Ignore systematic differences in explanation.
Limits	1. No longer really a comparison. 2. Difficult to establish causal relationships from monographs.	Difficulty in distinguishing what is due to context, to phenomenon studied compared to what is general.		1. Often extreme case study. 2. Risk of exaggerating differences.

Source: Vigour (2005:186; table developed from the book by M. Dogan and D. Pélassy 1982, fourth part).

Case and variable analyses

The specialist in comparison chooses between two approaches often compared: one which is first directed toward variables, and the other which concentrates on the thorough analysis of certain cases.

In the case approach, the comparison is based on meticulous examination and the comparison of several cases. The phenomenon is studied in its globality and diversity, using a historical perspective. The analysis stresses the complexity of relations of causality, as well as the national or historical anchoring of conceptual categories. Monika Steffen (2000) studies the transformations of health policies, following the AIDS epidemic in several European countries: Germany, France, Great Britain and Italy. For each country (even if she then represents the results in a comparative way), she draws up a chronology of these changes and explains the factors which have facilitated or slowed them. She considers both the epidemiological dimension – a historical and sociological perspective – particularly the relations between the government, doctors and prevention associations. Most comparative research really based on qualitative methods corresponds to this approach.

On the contrary, the variable approach is not related to cases per se, but it breaks them down into variables: the researcher identifies parameters which seem relevant to him given the issue he is studying; he then notes the presence or absence of each of them and their relative importance. Thus, to measure the degree of independence of central banks and scope of changes that they underwent during the decade of the 1990s, Yves Surel (2000) defines four principle variables (statute of the executive organ of the central bank, policy formulation, assigned objectives, limitations on loans to the government), themselves broken down into several indicators (for the first variable, the duration of the term, modalities of nomination of the executive, modalities of resignation, possibilities of holding several posts concurrently within the government). Each one of the variables being noted on a (maximum of independence) and the affected indicators of a value, Y. Surel, according to the modalities of operation of each bank registered, attributes to each parameter a value all the stronger as independence is better guaranteed; he can then compare the banks over time and between countries, based on the calculation of the degree of independence of each central bank on a given date. He shows that France has gone from a weak index to a high figure, slightly higher than that of the *Bundesbank* before the Maastricht Treaty; Italy has also seen a similar evolution but to a lesser degree. The author sees here the strengthening of a European institutional path which also influences even countries outside of the euro zone (in this case, Great Britain) through an effect of dissemination. The objective is to generalize, to test a theory all the while attempting to statistically control the parameters and possibly identifying deviant cases.¹⁸

With respect to method, the case approach – contextualized and often in part historical – grants great significance to configurations; it stresses the complexity of the causal relations discerned. On the contrary, variables analysis (often considered out of context) is often based on a statistical approach which depends on quantitative methods. In the first case, the sources mobilized are varied (written or oral) – whether it be interviews, archives, observation. In the second case, numerical indicators come from the mobilization of research and quantitative methods (even if it happens, as in the example mentioned above, that they were constituted from qualitative sources). Whereas the case analysis depends instead on a small number of units, the field of variable analysis tends to be broader. In a symmetrical way, the degree of abstraction is more reduced in the first approach than in the second; complexity and particularity are developed in the first approach, whereas variable analysis allows us to achieve a greater degree of generality and to obtain more synthetic results. The respective limits of each one of these approaches follows from this: on the one hand, the slightest ability to increase in generality and categories sometimes hardly transposable because of their contextualization; on the other hand, the absence of contextualization, the test of abstract hypotheses and *a priori*, without taking into account interrogations with respect to the reliability of data (to learn more, see the special issue of *Enquête*, 2001; Ragin 1989).

Conclusion

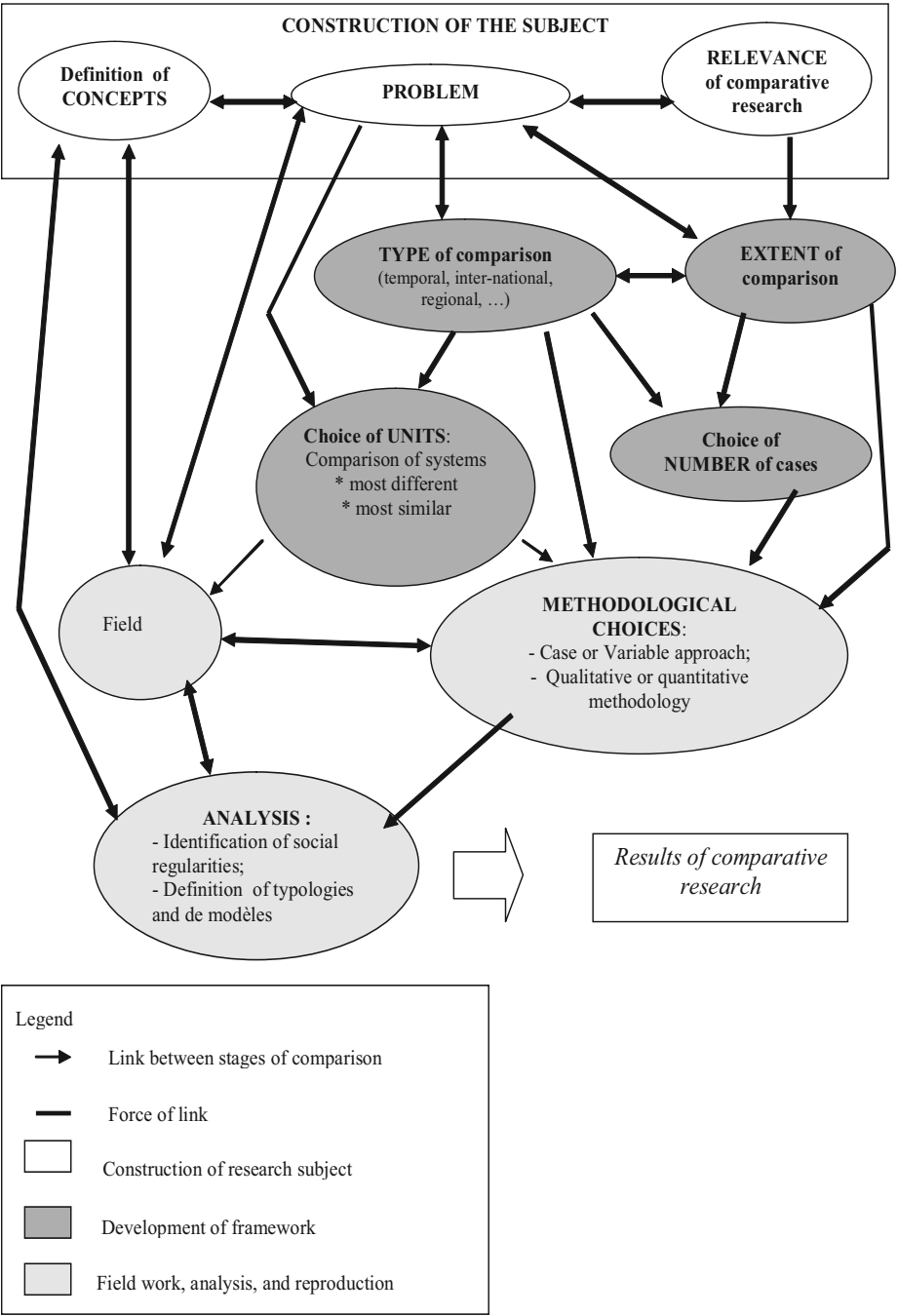
Here, in the guise of a synthesis are the main stages of a comparative approach, recapitulated in the following diagram and grouped into three important periods: construction of the subject, development of the comparative framework, and finally, field work and analysis. In practice, of course, several of them are overlapped, but here we have broken them down for greater clarity. Thus, the development of the framework for comparison is accomplished progressively, and in part, parallel to field work, following a come-and-go dynamic between the research phase, reading, and the work of formulation of hypotheses and development of a theory.

Table 4 – Ideal type distinction between case and variable analyses

Type of research	Case analysis	Variable analysis
Method	Historical, contextualized analysis, rather qualitative. Analyze configurations, stress on complexity of factors and causal relations.	Statistical approach, rather quantitative.. Analysis based on variables (considered outside their context).
Materials	Varied (interviews, archives or other written sources, observation).	Numerical statistics or indicators (possibly established from qualitative sources).
Number of cases	Often few	Large
Level of abstraction	Often more reduced.	High
Level of generality	Complexity, singularity.	Greater level of generality.
Limits	1. Less ability to increase in generality. 2. categories often hardly transposables, because they are too rooted in the fields.	1. Absence of contextualization. 2. Often test abstract hypotheses. 3. Reliability of data.
Research objectives	1. Formulate <i>hypotheses</i> . 2. <i>Test</i> the relevance of certain theories (study counter-examples, deviant cases). 3. <i>Detail</i> a theory.	1. Put forward <i>hypotheses</i> on large samples. 2. <i>Confirm</i> theories (test of soundness). Increase in generality easier.More synthetic results.

Sources: Vigour (2005: 205; Table developement from M. Dogan and D. Pélassy 1982 et C. Ragin 1987)

Schema 1 - Comparison in social sciences: the principal stages of work of the specialist in comparison



As we have seen, comparison is a stimulating research strategy for two main reasons. On the one hand, in confronting the researcher with the difference in near-by countries or on the contrary with similarities in countries if contrasted, comparison invites him to free himself from what seemed to him to be a given and broaden his field of observation and analysis. On the other hand, the work of understanding and explanation which is made more complex (by the relationship between distinctive details and explanatory factors) gains simultaneously in soundness because of its validation in several domains: depending on the number of cases studied, comparison enables us to advance hypotheses or to construct a model of analysis.

At the same time, to become involved in a comparative enterprise is not without dangers which are so many “downsides.” The main pitfall is with no doubt in the ethnocentric bias – namely, the fact of pinning a problem on other places, a model of analysis or a research protocol developed in a field. Another difficulty, and not the least, can be termed a “technicist bias”: the practical orientation of the comparison, which is ancient but which today is seeing a considerable boom through research on best practices (benchmarking, precondition for all new legislation, for example), runs the risk of distorting the understanding of the phenomenon studied, by the instrumental design which is adopted, on the one hand in leading the researcher to be interested in what should be, before observing what is, by encouraging him to stick to term-for-term comparisons (or terminological), without taking the exact context into which this social fact is inserted into consideration. This is the reason why the specialist in comparison should try to successfully distinguish systems of expertise from those of knowledge, and to identify the social and political issues inherent in the theme of his study the better to keep them at a distance.

Thus, we can explain the necessity of reflexivity for the specialist in comparison, understood as a distanced return back to his research practice. Some difficulties of the comparative approach indeed require a particular vigilance on the part of the researcher, facilitated by certain tools, like the rigorous definition of the research subject, central concepts and the problem; the ability to stand back (in relation to his preconceptions) without losing his abilities for empathy, the choice of cases to compare, etc. But that should not make us forget the importance of tinkering in concrete implementation of comparative research. These tools will not replace the experience acquired with practice. Depending on the characteristic of each field, the researcher is led to cope with the particular difficulties, because research always includes unforeseen situations which involve “tinkering” solutions.

Notes

1. Even if the comparative approach has been unequally mobilized depending on the disciplines and the time periods (Vigour 2005).
2. We will stress here international comparisons – certain methodological pitfalls being more pronounced in this case.
3. Certainly, if we define a method as a “set of approaches followed by the mind to discover and show the truth” or a “set of reasoned approaches, followed to arrive at an objective” (cf. the dictionary *Petit Robert* 1998), then comparison is indeed a method.
4. According to G. Jucquois and C. Vielle (2000), comparison is at once a methodology (by the mobilization of numerous methods), an epistemology (as means of knowledge for the comparison to the Other) and an ethic (faced with the risk of relativism).
5. Running the risk of placing fairly different cases in a same category. For a review of criticism directed at typologies, see Vigour (2005:287-291).
6. Other works are explicitly designed to answer a controversial question in the public space. This is the case of the work of C. Baudelot and R. Establet (1989), *The Level Is Increasing*, which was designed to show, with figures to back it up, that the French discourse on “the decrease in academic level” was erroneous (the average level was said to increase, even if the disparities were simultaneously sharply increased). The work is situated at the intersection of the research forum and the social forum.
7. Cf. also Delpeuch (2006); Dezalay and Garth (2002 and 2008). For analyses related both to the comparative approach and mechanisms of exchange and transfers, cf. Werner and Zimmermann (2003); Hassenteufel (2005) and Vigour (2008). For M. Werner and B. Zimmermann who call their approach crossed history, the reflection on intersections (practical and intellectuals) deals at the same time with subjects, viewpoint, (empirical and epistemological constitution of the subject), scales of observation and relationships between observer and subject (particularly the need to monitor the effects of asymmetry of relations between the research and his various fields or sources). (Inter) active and dynamic principle of intersections (unlike studies in terms of transfers), consequences and process-related dimension of these latter are dimensions essential to research. This research trend is one of those which has thought the most systematically about the theoretical, methodological and epistemological implications of an analysis of intersections. Developed starting in the mid-1990s, this multidisciplinary approach, the unity of which is based on the desire for historicization, aims to theorize, in proceeding by pragmatic and reflexive induction (cf. also Zimmermann, Didry and Wagner 1999).
8. Cf. the study on photographic practices in 1963 for Kodak by P. Bourdieu, L. Boltanski, R. Castel and J-C. Chamboredon. They drew from it *An Average Art*, where they highlight very distinct social uses of photography. From a commercial order, probably designed to better target the clientele, this team succeeded in making a sociological analysis.
9. Cf. M. Lamont and L. Thévenot (2000), who formed bi-national partners for the realization of research in such a way as to always introduce distance and a more “staggered” foreign regard.

10. Otherwise the risk is great to establish cats-dogs, i.e. non-relevant categories of analysis (Sartori: 1994). On the contrary, historicizing and making reflexivity prevail are means of questioning the seeming naturalness of all categories, and of articulating diachronic and synchronic dimensions (Werner and Zimmermann 2003).
11. This concept allows us to “link universals to particulars, to organize our categories on a scale of abstraction, whose basic rule of transformation (aggregation going up, specification coming down) is that the connotation (intention) and the denotation (extension) of concepts are inversely correlated. Thus, to make a concept more general, i.e. increase its capacity for mobility, we should reduce its characteristics or properties. On the other hand, to make a concept more specific (suitable with respect to the context), one should increase its properties or its characteristics” (Sartori 1994:32)
12. For example, we access the increase in salary associated with an extra year of studies, without considering the fact that salary varies over the career, depending on professional experience.
13. G. Peters (1998:51) speaks of a “selection bias.” Thus French researchers most often include France in their comparison.
14. For a synthesis of reflections on the game of scale conducted in social sciences, see Vigour (2009).
15. For a synthetic presentation, cf. Revel (1996) [hereafter designated by the abbreviation JXE].
16. As Pascal stresses, “a city, a countryside, at a distance, is a city and a countryside, but as we get closer, they are houses, trees, tiles, leaves, grass, ants, ants’ legs, *ad infinitum*” (Pascal, Blaise, 1963, *Pensées*, n°65-115, in *Œuvres complètes/Complete Works*, Lafuma, Paris, Seuil, p. 508 – cited by Bernard Lepetit, “De l’échelle dans histoire/On scale in history”, in JXE, p. 94).
17. The question of whether the main point in research is to make the scales vary, or even adopt a micro sort of approach remains controversial among researchers whose work refers to micro-analysis. For J. Revel, “no scale is privileged over another, since it is their comparison which procures the strongest analytical benefit.” For others (like M. Gribaudi), “in the production of forms and social relations, the “micro” engenders the “macro”; the result then is “an absolute privilege of the first”. Cf. the study on photographic practices conducted in 1963 for Kodak by P. Bourdieu, L. Boltanski, R. Castel and J-C. Chamboredon. They drew from it *An Average Art*, where they highlight very distinct social uses of photography. From a commercial order, probably designed to better target the clientele, this team succeeded in making a sociological analysis.
18. There are intermediate cases, like the longitudinal cases, dealing with a large number of countries. It is also possible to associate the two approaches (cf. Boolean analysis developed by Ragin 1987).

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13

Writing in the Social Sciences: From Field Notes to Scientific Reports*

Alexandra Bidet and Erwan Le Méner

The researcher in the social sciences never stops writing. To size up the activity of writing which continually accompanies research is to follow science to the place where it is developing. Writing is far from being a transparent tool,¹ a simple mode of expression, which steps in only at the end of research to make the results public. Given that the sociologist or anthropologist spends the majority of his time writing notes, memos, transcriptions, articles, etc., it is remarkable that he is scarcely taught how to do so, and that he talks very little about it, as if his reports were like ripe fruit falling from the tree.

Nevertheless, some practitioners are interested in the scriptural production in the social sciences. The works of Jack Goody (1979) throw some light on the coextensive character of science and writing. The interest of the famous anthropologist in the invention of the printing press led him to study the concrete modifications associated with the appearance of writing: what are the “powers” of writing which, for him, begin from the list or the table? What are the cognitive effects of these graphic inscriptions? The invention of the printing press appears at the beginning of the birth of a critical tradition: with the printing press, texts began to circulate in a crystallized, fixed form, freed from their authors and from the time of their formulation. No longer was it a matter of forms of knowledge constantly in movement as in the oral tradition, where they are also partially recreated at the same time as they are transmitted, but to a stable reference, provided to view, which can then be compared, examined, criticized: we can write other texts referring to them, and a critical discussion can be developed about them. “Powers of writing” depend on the very possibility of a systematic, reflexive, and cumulative knowledge.

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Taking writing seriously encourages us to consider, after Charles Wright Mills (1959) and Howard S. Becker (1986) particularly, that to be trained (or train oneself) in research in social sciences, is to be trained (or train oneself) in the author's craft. The insistence on the concept of craft indicates two things: the researcher produces mainly reports most often writings (Latour 2006); and writing is *worked on*, it assumes a constant effort, and not a preordered group of ingredients as in a recipe. Paying attention to the question of writing is thus to ensure a constant critical awakening, particular to "the diehard empiricism" (Schwartz 1990) of the social sciences. This does not suggest skepticism but, on the contrary, a concern for rigor and sociological imagination as to our formats of writing, our styles and our vocabularies.

The first official report is, however, more unrefined, and could take away beginners' feelings of guilt: writing does not come easily; few and far between are the sociologists who do not have difficulties with writing. It is often seen as a test, a jump— the "transition to writing"— more than as background work, inscribed in time and in a system of discovery. Can we hope to come to terms with writing? In this contribution, we assemble some main proposals to this end. It is a question of both flushing out the lures which hinder writing, make it intimidating and discouraging, and of making a list of "tricks of writing" already explored by researchers that the reader could then test and enrich in turn.

Some False Ideas on the Subject of Writing

These ideas consist in thinking of writing as the simple putting down in words of a thought which is already clear, or results of research developed previously. Writing is thought to be outside of the core of scientific activity, and its qualities are of little importance, no more than they are cultivated. Among the myths that harm our practices of writing, we find both that of writing as a "personal talent" or of "easy writing," the belief in the existence of a "single good way to write" that the "best researchers" are thought to possess and that has only to be discovered. In all cases, it is the very nature of writing as work which is misunderstood.

The myth of writing as an innate or superfluous ability

If "everyone knows that sociologists write very poorly" (Becker 1986), that they often multiply unnecessarily complicated terms, long and bombastic sentences, we could infer that the talent of writing is not among the professional abilities normally required of sociologists. Writing is said to be only a "plus", a supplement to "style", principally ornamental, which some are lucky enough to have a talent for and others not. The context of the text does not depend on its form, as if the core of the research was over before any reproduction in the form of a report.

There is a double misunderstanding here: on the one hand, respect for the norms of writing most often plays an important role in practice in the way that

editorial committees of reviews assess articles; on the other hand, this technical and differentiated language, this “academic prose” that beginners perceive as the very mark of the “science,” can be the subject of training. Beginning researchers do not hesitate to imitate this style, thereby giving credit to the idea that there are not other legitimate ways to write, to “do science,” than this academic p(r)ose. And if it is important, in particular to publish, to master all the variations of the academic style – each review, each university setting having its own requirements and formats (number of characters, text format, expository mode, etc.) – it is also important to not become obsessed with it, nor become a prisoner of it, especially at the first stages of research. And even the apparent absence of style can resemble a form of style. Finally, in reducing ethnographic writing too much to that of a final report, we run the risk of losing sight of the multitude of writings that came before this document. However, these minor documents are not only “intermediaries,” to repeat a distinction made by B. Latour (2006), i.e. *inputs*, but especially “mediators,” i.e. elements which direct the text which will result in an often unexpected way.

The myth of “easy writing” for others

Here, H. S. Becker attempts to set us straight: nothing is more common, among researchers, beginners, students, but also senior researchers, than the anguish of the blank page. The final draft, in particular, can be hindered by the consciousness of the issues of a text which will reveal to the external world the existence of a work. A part of the problem of scientific writing thus comes from the very institutions of intellectual life and of their community of judgments. To complete a text is to both “congeal” a reality and to open oneself up to criticism, commentary, and suspicion. The regulation of scientific production assumes this peer review: to render an account of the development of one's research is to allow oneself to be told that it could have been done otherwise. Indeed, H. S. Becker states and criticizes the multiple strategies of production currently adopted by researchers to protect themselves against such judgments: beyond the “blank page” or incomplete text, they can still hide behind wording which is excessively cautious, alembic, abstract; and they often have to increase the number of little reassuring rituals to be able to start writing.²

Among these strategies of “protection,” we also see the various ways of claiming a form of authority. A part of our writing strategies are thus a quasi-ritualistic way of “telling our readers who we are and why we should be believed...”. Among the “persona” the most frequently claimed, is to point the authority of experience (I was there and I saw is the classic authority of the anthropologist à la Malinowski), the one who argues from an ability of an initiate (profusion of scholarship, excessive accumulation of scholarly details), or the one which intimates, in and by the text, that no matter who would have seen as

much.³ If it is important to deconstruct these classical figures of ethnographic authority, it is not so much to show the scientific fragility of the scientific report as its artificiality: writing is not at all natural, is rarely a given, and implies the deployment of efforts, of reasoning, of strategies which, far from weakening the argument, instead extends its reach.

If the student, tempted by mimetism, often aspires to slip into one of the *persona* offered at a given time in his academic space, the researcher is often tempted by “representation”; his stylistic choices, more ritualistic than semantic, underline an allegiance to such or such “trend” or “clique,” and mark the adoption of a pre-written role in academic space. The desire to say “I am a functionalist” can, for example, prevail over that of saying what one really wants to mean. The spread of these idiosyncrasies, which sometimes thwart the requirement of promotion of scientific writing, shows once again that writing is not generally an easy activity.

The myth of a “good way” of writing

Another erroneous idea would have it that in writing there are models or rules to follow which can allow us to avoid all difficulties. The most pernicious model, because we often follow it without even realizing that we are doing is, is that of the academic exercise, the “written test”: this idea would have us believe that the best technique, that of both the brilliant author and the good student, consists in writing quickly, at one sitting, in one go. Any starting over on the text, any “crossing out,” is thought to be a sign of “cheating” or imperfection. Our academic habits, the result of serious training, pushes us to disdain rewriting which is, however, essential to scientific writing. The research thus becomes an often disconcerting activity for the beginner: it requires a whole other training, and accepts assessment criteria which are quite different from those of academic exercises. A “too perfect” work, in which we see only the endpoint, could, for example, be judged less favorably than a work which shows the reader its development, the questions which remain, the mistakes, misunderstandings or prejudices which were removed, the redirection that was necessary, etc.

The influence of the academic “model” certainly plays a role in the a proof of personal worth, the school system “paralysis” which often takes over faced with writing. By making academic papers a proof of personal worth, the school system has tended to transform writing into an acid test which determines whether we are a winner or a loser. Schools do not schedule such papers as a fun or practical exercise, but as a public and irrevocable test of identity and personal value; this is all we need to encourage blocks or procrastination which constantly puts off the test. As Clifford Geertz reminds us, the most virulent objection, widespread in practically all contemporary intellectual milieus, attests that “stressing the way in which assertions of knowledge are presented” is said to harm our

ability to take its assertions seriously (Geertz, 1996, p. 10). Dismantling finely wrought texts would amount to accepting the relativism of knowledge. According to Geertz, on the contrary, it is extremely important to understand how the texts are made and readers convinced in order to understand “the criteria enabling us to assess them” (Ibid, p. 14). Deconstructing ethnographic writing would enable us to better objectivize our judgments and knowledge, to better relate to them, and indeed to encourage communication between peoples.⁴

Training in research in the social sciences is then that of a totally different relationship to writing: writing as constant work, and a work of an artisan more than of an artist. To play down the activity of writing, we then need to go further: not only to refute false representations, but to transform its concrete relationship to writing: try it out as the daily place of a system of discovery.

Writing as Work: The Researcher’s Workshop and the System of Discovery

In observing the researcher in social sciences, we cannot overlook noting the multitude of writings which surround him: scientific articles which he writes are part of a vast group of texts – index cards, notebooks, and notes of all sorts – to which we can also add writings of colleagues, classic texts, periodicals, etc. The researcher in social sciences has everything to gain by seeing himself as a professional when it comes to writing, just as much as in the production of ideas or research.

Write, always write. Why keep notebooks, index cards, memos?

To play down writing, there is nothing better than to increase the number of “little writings,” seemingly minor or without a particular issue. In the well-known appendix to *L’imagination sociologique* [*The Sociological Imagination*], “Le métier d’intellectuel” [“The Profession of Intellectual”], C. W. Mills recommends all-out writing of index cards: “index cards are to the sociologist what notebooks are to a writer. They are indispensable” (Mills 1967:200). First, let us clarify that index cards – which can also be notepads, notebooks, electronic files, etc. – are the best way to get into the habit of writing: “you run the risk of getting rusty if you do not write at least once every week.” Accumulating index cards means that you are practicing writing, gaining ease, making the writing a daily experience and not something exceptional. C. W. Mills suggest that the intellectual artisan subdivide his index cards: ideas, personal notes, reading notes, bibliography, proposals, etc. Keeping them up to date, consulting and sorting them regularly also “maintains an internal alert”: that encourages “catching an understanding of ideas, observations, words” which, “once set down on paper, can be useful for more reflective thought”, and maintain “free attention” (I. Joseph) which lends itself to seeing new things; their manipulation helps us on a daily basis to get out of our mental

routines, by showing up unexpected connections, “unsuspected relationships” between notations made at different times. This makes for the effectiveness of the written word. Just as photographers would not walk around for anything in the world without their camera, fearing that they might miss a successful shot, sociologists should not go out with a pen and notebook stuck in their bag!

C. W. Mills stresses in particular the importance of reading notes and advises us to get used to taking copious notes each time we read a work or an article. This is to save time and energy – we can refer back to them more easily later – but also a savings in time for analysis: note-taking, but also underlining, commenting, or summarizing in the margins, etc. encourages active reading, cultivating a suspicious, indeed disrespectful relationship to the text. A book in the social sciences does not read like a novel: at the same time that the reader tries to relink the questioning of the author to his own and assess his contributions, he puts him to the test by reflecting on his flaws, on criticism that can be addressed to him, questions that he leaves open and research that they assume, etc. He can read only some passages that he rereads many times without necessarily going through a linear or complete reading of the work. But he never limits himself to some quotations. To read is, on the contrary, *to reread* and *link together*; to this end, we always read and give priority to the introduction and the conclusion, but also the table of contents and the index to understand the logic of the whole subject and its “overarching idea,” and the bibliography to clarify the intellectual universe of the work, and to complete its list of books to “skim” or “read.” Finally, we should always read the footnotes meticulously; they are very useful for understanding the most subtle aspects of the position of the author as such may deserve a reading unto themselves.

Defenders of grounded theory consider writing and the frequent consultation of memos with analysis based on field work inscribed from the beginning of the research process to be indispensable to discovery. The comparison of memos, their blacklisting, already recommended by W. F. Whyte in his postface to *Street Corner Society* (1955), enables us to distinguish categories under which observations will be subsumed and then linked. Memos can, furthermore, make up a system of communication and coordination between members of a research team, today all the easier with the widespread use of the Internet, allowing for exchanging ideas at a distance.

More generally, these multiple little bits of writing, and their frequent rereading, bring to daily work the miracle of the “good issue” or the “good subject,” that students otherwise search for eternally in the heaven of ideas. Keeping index cards or notebooks promotes the development of fields, subjects, problems, while allowing daily experiences to fuel research as well by hybridizing with other notations. The metaphysical question, “how do ideas come?” then becomes: “how to encourage and catch ideas?” It is with index cards that the sociological imagination is cultivated, which distinguishes, according to C. W. Mills, the “good

worker” from the technician who is “too educated”: a veritable training in agility and sociological curiosity, index cards train us to change perspective, to diversify points of view, and not to give priority to sorting out thought on the extent of one’s questioning. They teach us to think by successive strokes and not all in one piece and all at once.

Field notes: Why “note everything”?

To students who worry about the right way to deal with their field journal – what amount of detail? What amount of analysis? – H. S. Becker answers by an instruction that appears to be puzzling at first: “note everything” (1998, pp. 131–146). But can we note everything? Obviously not, the sociologist immediately assures us. Far from the fantasy of an “entirely complete description” or exhaustivity in data collection, he only sees in this the regulating idea, a “sensitizing concept” H. Blumer would say. One thing is certain: it is possible to note many more things than we do spontaneously and this will always be: more exact than a description that leaves lots of things aside.” Otherwise, we can always tell ourselves after the fact that we could and should have taken more notes, that we miss some things that escaped when we had them at hand. This is what happened, for example, to W. F. Whyte (1943), after several months of research in Cornerville, when he realized, during a bowling game, that the core of his observations was the organization of gangs, and the neighborhood on the whole, and that he had unfortunately not kept any written record. It is also this which encouraged M. Duneier (1999) to extend his analysis of newspapers vendors to the larger world of the sidewalks of Greenwich Village and start his field work again from scratch, after his main informant, H. Hasan, read a first draft of his study, and pointed out to him that it was too focused on his own idea of the vendor. M. Duneier then completely restarted his research by taking the newspaper stand of H. Hasan, who turned it over to him in his absence, as a post for direct observation and recording. Like others, M. Duneier indicates the advantage of recording – in writing, by audio or video tape – at the time, in the heat of the interaction, or as little time as possible afterwards, the greatest number of elements of what is going on.

The researcher is most often ambivalent with respect to writing: on the one hand, it marks his marginality on the social scenes studied – the situation of the *outsider*, who watches and listens *in order to* write, often being synonymous with inconvenience, isolation, even alienation; on the other hand, it allows him to find himself in this role of researcher, in relation with the academic community. After the stimulation and fatigue of the field, it is in most cases a burden for the most part. Thus, in the field, it is important to find ploys to get away for a few moments and take down *a minima* some key words for the record, as well as a maximum number of expressions, terms and exchanges with natives (that we will carefully note in quotes); taking a sample of selected items (photos, docu-

ments, etc.) will also help in the exercise of memory by enabling us to “resee” the scene. The rapidity with which the notes are completed, commented on, and clarified (at the beginning, counting on an hour of work in the office, for an hour in the field) will determine their richness, their specificity and their accuracy because, obviously, the longer we put off writing, the less and less well we describe. D. Cefaï, in his courses on training on field research, speaks of the “memory like a sieve.” At the beginning of field work, abundant and detailed notes on the ways of speaking and doing, the way in which one is welcomed and treated, but also the places, sounds, colors, objects movements, atmospheres, etc. are all the more valuable because the intensity will become dull with familiarity. Next, empathy often takes over. It will then enable us to perceive what is significant or important in such or such a situation for those being questioned: what are they attentive to? What makes them react strongly? What they looking at ? What are they talking about ? What constitutes an “incident”, “trouble” or “problem” ? What do they do about it? How do they assess or comment on what has happened talking to the observer? etc.

In order to show students the usefulness of the advice to note everything, and initiate them in note-taking, we can show an ethnographic film and ask them to “note everything” and then compare what each one has actually noted. It is always a big surprise for them to discover the extent and diversity of what can be noted. The exercise also allows us to show the relevance of accompanying the raw description with more personal notes (reactions, emotions, associations of ideas, links with academic reference or others, etc.) and, on the other extreme, of thought about the approach and the preconceptions of the film director and/or ethnographer.

This trick again allows us to not give in to panic when confronted with two possible experiences of the research: one being to consider that what one observes as much weaker than expected; the other being to falter under the overabundance of the real. In one case, noting down everything can bring out motives that had gone unnoticed, which gradually sketch out a framework in the research. In the other case, noting down everything can bring out motives interwoven in a denser fabric and concentrate attention on a particular point, whereas the time is often limited and the centrifugal relevance of the world observed can engender a certain paralysis.

But “noting everything” is also *noting otherwise*. H. S. Becker draws our attention to the spontaneous tendency of beginning researchers to substitute “analytical summaries” of what has been observed, already rich in implicit interpretations (and therefore conventional): “what they believe to be raw descriptions are usually nothing of the sort, but rather sorts of analytical summaries of what they have seen, summaries developed to avoid the requirement of noting everything” (Becker 1986: 132). A global judgment, “it was sad,” for example, will replace a detailed description of the situation. An attempt at specifying the cause of an

emotion will, likewise, short-circuit the description of its appearances, reactions it evokes in the participants, etc. To harvest rich data, the sociologist has everything to gain, as in conducting interviews, by giving priority to the “how” over the “why”, and by systematically retranslating his or her research questions in descriptive and not explicative terms: not “why does this group exist?” but “how was it formed?”

Asking students to “take note of everything”, thus trains them in the art of enumerating without commenting, without trying to infer motives or intentions of persons, thus the flip side of ordinary experience. We are not accrediting here the myth of a “pure description,” but observing that there are descriptions less interpretive than others and that these simpler, less analyzed observations teach us much more. There are several ways to take notes in the field, some in first person, giving free reign to emotional flow, evaluative scanning, and to an exacerbated reflexivity, at the risk sometimes of making the researcher the main subject of the research.⁵ The instruction suggesting that we take note of everything is not aimed at reducing this variety of approaches, but rather indicating that the real is always richer and more amazing than we suspect, and that our propensity for curiosity can be too quickly stifled by the too rapid insinuation of interpretations and analyses which format our perception without our knowing and in an often definitive way. To protect ourselves against an excess of interpretation on notes in the field, we can, for example, maintain a strict typographic separation of what is part of description and what is part of interpretation, by only writing one or two pages of notes in the field, leaving space opposite free for all sorts of wild imaginings that one doesn’t want to either forget or lose on the fly – and which will prove to be extremely valuable later on for some! (Beaud and Weber 1998).

“Noting everything”, subjecting ourselves to “massively detailed descriptions”, gives us, on the contrary, the possibility of being surprised by one’s data and of forming less conventional interpretations: “a meticulous description of details which does not go through the filter of our ideas and theories produces observations which do not tally with these categories, and which thereby require us to develop new ideas and new categories that they could be integrated into without forcing” (*ibid*: 146). Conversely, without this effort, the risk is great that the researcher will only renew his prior schemes of perception of the social world, whether they be scholarly or indigenous: “We see things which we already have ideas about, and we cannot see things for the description of which we have no word or no idea” (*ibid*: 48).

This effort to push back the interpretive moment is among the most difficult: nothing is more reassuring than to interpret, to immediately associate what we observe to sociological readings, at the risk of turning oneself into an “observer or ethnographer going by the book.” First, this avoids the unsettling effect of surprise (we see what we came to look for; we find what we already knew); and then, we immediately have the impression of doing science (that “seems”

sociological)”: “sociologists expect interpretations from themselves and their colleagues. In general, they want to reduce the quantity of material that they will have to handle, by seeing in it illustrations or proofs of ideas that they have, and not something that has to be displayed in large quantity for its particular interest,” H. S. Becker laments on this point. If the over-interpretation (Lahire 1996) or imposition of the problem (Duneier 2006) are the main pitfalls of field work against which the researcher should steel himself the most, all the “tricks” that H. S. Becker provides are designed precisely to support this effort.

“Note everything” is one of these tricks which, by encouraging us to produce richer data, should promote the invention of “new ways of manipulating things”—questions, possibilities of comparison, categories, etc. — they alone able to advance knowledge. The same is true of the trick which suggests that we “see people as activities” and not only to “type” them: “putting people in a category is a way of rendering an account of the regularity of their acts (...) by concentrating on activities rather than people, we force ourselves to be interested in change rather than stability, in the concept of process more than that of structure” (*ibid*: 86). This allows for richer descriptions, but we also avoid immediately limiting the field of observables: “starting with activities allows us to focus the analysis on the situation in which such an activity takes place and on all the connections that your subject of study maintains with the things that surround him, i.e. with his context.”

In *Qu'est-ce que la sociologie? [What Is Sociology?]* (1970), Norbert Elias formulates an additional trick: a chain of personal pronouns. Because they are present in all human groups, this reminds us that we cannot represent an “I” without a “you,” a “he,” a “we,” etc. To produce rich descriptions, attentive to the network of interrelations which weave every individual into a “man among men” and not as “*homo clausus*,” this trick involves systematically multiplying the points of view. In the same vein, Georg Simmel suggests that we no longer see the ego in the middle of concentric circles, but an intersection of innumerable social influences. In both cases, the challenge is to develop relational concepts. The metaphors proposed by N. Elias (the configuration of players, the weft of a fabric, etc.) is thus designed to help us to thwart routines of thought inscribed in the vocabulary and syntax of languages marked by Aristotelian categories (matter/form, substance/attributes). These encourage us not to only develop static, reified subjects, and not to only give a secondary status to movement and relation. In “the river flows,” the flowing is external, as added to the inert river. We assess the arbitrary and the perverse effects of such a mode of representation. On the contrary, the researcher should, as much as possible, multiply and precisely define the various perspectives and wonder who smells, says, thinks, judges what, according to such or such inscription in a configuration of actions. To render an account of the intrinsic dynamic and relational character of social life is thus a constant challenge for the researcher, who confronts this in and by writing.

Attention to writing is at the core an effort at detachment – that N. Elias evokes by the image of the spiral staircase, which we see ourselves going up (1970). From index cards and field notes to scientific articles, watching ourselves in the process of writing allows us to become aware of choices, implicit preconceptions (with whom did I identify the most easily? What did I pay attention to?), to examine their limits, and to counteract them if necessary. We should remember that we can take notes without making hypotheses on what is important to note, i.e. on what will interest future readers, but also on what is significant, important for the subjects of the research themselves. Clarifying these hypotheses, by rereading field notes with a critical eye, means that we are erecting a rampart against ethnocentrism or sociologism; it will allow us to return to the field with increased curiosity, and to no longer shut ourselves up in a pre-constructed problem – the temptation of the sociologist being, for example, not to pay attention to the effects of social hierarchy or “symbolic” dimensions of activities studied. Thus, we understand that writing is always re-writing.

Writing is rewriting: Why multiply the versions and let them be read?

Writing is the workshop of the social science researcher, the place where he becomes aware, during all of his research, of problems to resolve or resolved; he runs into choices, implicit or explicit, sees what he has at his disposal, develops and experiments with descriptions, puts his ideas in order and works on their (re)formulation. If it is important to begin to write reports on his research as soon as possible,⁶ it is because writing has a major role in discovery: contrary to what is often believed, we never know in advance what we are going to write, what will finally come out of our confrontation with writing. Bruno Latour reminds us of this: “In our discipline, the text is not a story, a beautiful story; it is the functional equivalent of the laboratory. It is where we do tests, experiments and simulations” (2006). Training in the craft of researcher is thus that of in the trade of author, producer of “dangerous reports”: “Not teaching doctoral students to write their dissertation is like not teaching chemists to do experiments. This is why from now on, I do not learn any else but writing (it’s true, I end up giving myself the impression of being an old idiot who always repeats the same thing: “describe, write, describe, write”)” (*ibid*).

As a result, writing is above all rewriting. In fact, all problems are not resolved at once, starting with the first version; the first sketch is never the good one. To say that rewriting is the true work of writing is to also say that there is cause to look for the “right starting point,” the right way to begin, which often paralyzes us faced with a blank page or the screen. The writing workshops that H.S. Becker organized for his students were aimed precisely at showing them, starting from their own texts, that the issue of the continuous work of rewriting is the very development of their thought: not a concern for elegance, a supplement of “style”

but the way to find what we want to say, to develop our argumentation, to test it and hone it, and be able to express it in a clearer, more concise and convincing way, by avoiding useless words or inferring “false profundity.” These workshops allow for experimentation with the unsuspected quantity of work that rewriting and correcting a text require, and the fact that this massive correction of “details” is worth the effort to do. In economics, the “o-ring” theory (Kremer 1993) attests to the utility – particularly for the quality of the finished product – of calling on highly-qualified labor at all stages of the production process, including some tasks of minor monitoring, but the elimination of which can have catastrophic effects on the consumer and, in turn, on the producer.⁷ Likewise, without extreme vigilance exercised over each detail of the text, the credibility and verisimilitude,⁸ the quality of the report, are threatened. The soundness of an article sometimes depends on very little – for example, a simple description!

The idea is valid also, before the correction of the first analyses, for the work on the field notes. Upon a close reading of them, questions of observations appear which lead us to redo the field research to complement our corpus, whereas we thought that this part of the job was finished. Although field notebooks are very often private objects that we do not like to show because they contain research like a private life, but also rough, primitive notations that we think are not worthwhile, they pay off, however, by being discussed on the actual evidence. The workshops on reading field notes by N. Eliasoph and P. Lichterman, in Los Angeles, those of A. Cottureau and S. Baciocchi, or C. Gayet in Paris, bring together students and researchers on rough retranscription of observation sessions. They prove to be time well spent to move the research forward, and discover heretofore unexplored, but perceptible paths between the lines, so long as we agree to have colleagues come to the workshop.

This “cooperative” dimension (Joseph 2007) of research is basically epistemological: research involves interacting with an environment which holds answers in order to continue to act (Dewey 1993) and there is a certain masochism in depriving oneself of the entourage of close friends and colleagues who have agreed to read you and comment, whether it be on the weak stylistic quality of your notes, or a proprietary concern. On the one hand, forgetting that the heuristic character of notes depends much less on their formatting than on their descriptive precision (who is speaking? when? where? with whom? about what? from which empirical viewpoint am I speaking as I am reporting?). On the other hand, it is naïve to believe that the collective discussion of a work in progress undermines the authority of the researcher. Strictly speaking, he remains well and truly the author of texts to appear (there is little chance that his readers will agree to take on in his stead the “dirty work” of writing, according to the still frequent prejudice!); and especially, a text never exists except as interwoven in other texts and discussions that he cites, comments on, summarizes, critiques, as so much expressive support, as Jorge Luis Borges or Gérard Genette have already shown

long ago. In short, there is a certain value in considering the discussion of our materials as just one point among many in the research, and not as a test of style or a burglary.

The argument is simple: to write is to think, by versions and variations. We cannot think “all at once”: thinking is a dialogue with the data, multiples writings, colleagues and oneself. Like any creative act, it assumes a duration; there is a temporality which is peculiar to writing, where downtime is as important as periods when we write our three pages of text per day. The work of writing, with the concentration that it requires, the settling that should often precede it, does not follow a linear temporality. Sometimes we write a lot, and other times not at all. Furthermore, it is good to let a work that is almost finished “sit” and then take it up again with a fresh look after several days or weeks. Finally, we should once again stress the interweaving between reading and writing: all rewriting assumes re-reading. No successive versions without constant re-reading, by the researcher himself – who is obviously his first re-reader – but also by trusted readers to whom he does not hesitate to entrust his first drafts. C. W. Mills thus stresses the value of “presentation contexts”: presenting our ideas to others, in the framework of informal discussion, research workshops or seminars requires us to specify them, clarify them and make them as accessible as possible. The reading by someone else and re-reading are worthwhile as chances for testing, control and critique (in the Kantian sense, of work on the limits of validity) of the subject. A “presentation context” thus always tends to turn into a “new context for discovery.”

For all that, even before the intervention of “rereaders,” writing is thus a collective activity, part of a framework of discussions. B. Latour illustrates this quite well in describing this researcher specializing in soil analysis who, in the middle of the Amazon forest, carries his dear colleagues “in his stomach”: the importance of the way in which he is going to make the holes cannot be understood outside the imaginary – but no less real – dialogue held with colleagues thousands of kilometers away (Latour 1995:51).

Some “Tricks” of the Writer’s Craft

By outlining his “tricks” of writing, H. S. Becker does not intend to state “rules to consciously follow,” but to encourage discussion and experimentation of each person on fruitful practices of writing. His “tricks” call for a pursuit of the inventory of techniques developed by researchers and to discuss more than we do the modalities and strategies of scientific writing, of its “styles,” which are still today rarely debated, regardless of the country considered. The question is, however, essential: the public character – debatable and shareable – of our productions depends on our ways of writing, presenting arguments, showing, illustrating, within the scientific community, but also in society itself. Can we really convince

without showing the reader specific data and not simple “illustrative vignettes”? How can the causal inference be based on what J. Katz calls “luminous descriptions”? What are the formats and formulas through which we can initiate discussions and make our proofs tangible?

Classify, sort, analyze notes

We would like to give some tricks here for dealing with the possible overabundance of writing of all kinds (field notes, log notes, memos, etc.), which are not yet final reports of field work. We are not concerned here with the style of notation, but rather classification strategies.

For the moment, we have tried to denaturalize writing, and present it as an activity necessary at all times in research (and not only in production of the final report), as a fastidious activity (in the sense that we should “note everything”), but also as an activity which depends on the knowledge produced (because the text is a laboratory). We have particularly stressed the primordial importance of field notes. We have thus suggested that you write, again and always, at the risk of giving in to feelings of frustration, distress, even exhaustion, faced with the quantity of materials amassed throughout the chain of writing which makes up sociological work. What then to do with the mass of data collected? What to do with all these notes which obviously do not all allow us or completely answer the very questions that we ask ourselves? How to read, sort, classify, and analyze? We will quickly present some classic suggestions which share a common analytic, inductive approach, taking the greatest care to very precisely link the analyses to graphic inscriptions from field work.

The main part of the subject is dedicated to grounded theory, a trend embodied in particular by A. Strauss and B. Glaser (1967),⁹ which played a fundamental role in the United States starting in the 1960s in the epistemological and methodological legitimization of qualitative sociology, in an academic world dominated at the time by quantitative research (survey research).

While leaving aside the very moment of writing and note-taking, grounded theory provides very fruitful leads for extricating oneself successfully from the corpus of notations from field work. From a general standpoint, it rejects all hypothetical-deductive approaches of field work and instead sets forth an abductive approach where production, analysis and theorization of data alternate, complement each other and deepen mutually.” There are not two phases,” write A. Strauss and J. Corbin (1990:365), “one of formulation, the other of falsification of hypotheses, but a double movement of generation of codes, categories, of their properties and their relations in data analysis, and simultaneously, the testing of their validity, elimination of negative cases, modification of qualitative samples, controlled refinement of identities and differences up to the point of saturation.”

Grounded theory has, in particular, further perfected procedures of data coding, including field notes. It distinguishes several phases of coding. First, it recommends an analytical reading and dissection of the corpus, line by line, according to categories the production of which is done, obviously, by groping about, as we better define what precisely we wish to study. Then the researcher is invited to return to the field and formulate “generative and comparative questions which expand the research guide of the researcher on his investigation sites” (ibid: 373), and to specify as result and by comparison the analytical categories discovered before. This operation is called “open coding,” opposed to “axial coding” and “selective coding.” “Axial coding” consists of better understanding the existing relationships between the various categories of analysis of the action studied. It is particularly concerned with distinguishing contexts of emergence and behavior of certain phenomena and questioning their relationship. Finally, “selective coding” is “the process by which *all* categories are unified around central categories,” whereas the categories which require further explanation are fleshed out with descriptive details” (p. 375). Coding is complete when data production and analysis no longer lead to discoveries and reach a “saturation” point.

This is an approach which claims, in perhaps a bit naive way (Burawoy 1998), to be largely inductive, to generate knowledge from the field. The theory – what must be explained and the way in which to explain it – emerges in the field and does not pre-date it; only incremental production and analysis of the corpus give it form. Even among its partisans, many criticize grounded theory as not very defensible scientism, but recognize that it has the merit of having opened up reflection on the tools, stages, procedures of production of knowledge in a qualitative approach.

In a fairly close vein to grounded theory, representatives of analytic induction give some useful tips to make sense of the mass of materials accumulated in the course of research.¹⁰ They consider that work on the data consists of a permanent redefinition of the question of research and explicative elements, until a locking of “theory” is accomplished, meaning that all cases to be explained are done so by the same explanation. In other words, the analysis of materials is a readjustment not only of the importance of the activity, of the phenomena, of the fact to be explained, but also of explicative factors, as the analysis discovers negative cases, i.e. not explicable in the envisaged way, or the consideration of which leads us to redefine the spectrum of experiences to study. The driving force of analytic induction is the discovery of negative cases in the corpus. These cases resist the explanation, which up until then explained other cases, without, however, leaving the field of study. Analytic induction is particularly useful for describing careers, and their stages, i.e. the processes without which the observed phenomenon happens.¹¹

Thus, grounded theory, as analytical induction, gives us leads for giving meaning and order to the mass of data accumulated during field work. Once these data

are coded, and “explanatory theories” formulated, it is then necessary to render an account of these new analytical proposals, by producing descriptions which will shed light on the causal inferences detected in analysis of the corpus.

How to write the argument – between description and explanation?

Once the notes have been reviewed, the memos sealed, the materials analyzed, writing the report is facilitated, but still sprinkled with tests that we can regroup under this question; how to write the argument? Counterintuitively, perhaps, but in accordance with all the work on the data previously completed, we must once again trust the description, much more than the explanation. When great sociological problems overpower the author, the best thing to do is probably to refer to descriptions contained in notes, and selected during their analysis, rather than to call on explanations which short-circuit the field and which reach out to each all the more in that our knowledge of the field is important. In other words, as B. Latour (2006) suggests, with respect to the descriptive and political point of view, we should again slow down on the path of explanation when we see it emerging on the horizon.

As long as possible, we should strictly connect our inferences to detailed descriptions. These descriptions are obviously not verifiable, but they will be all the more realistic and credible, as M. Hammersley (1993) stressed, as they are explicitly situated within a corpus, positioned within an argument, and make public their production constraints. Thus, their quality will depend largely on their fallibility.

To fight against the blank-page syndrome, several authors recommend mobilizing the resources of rhetoric, in a perspective which is no less realistic. We must suspend the demand for an explanation (the why?) by making the description (the how?) intriguing. In a series of recent papers, J. Katz suggests formulating a certain number of tricks so that we “[qualify descriptions] as “revealing,” “colorful,” “vivid,” “poignant,” or “strategic,” “of great richness,” “dense in texture,” or “finely qualified.” They are developed because they show how conducts are “crafted,” “anchored” and “situated” or because they contain “paradoxes” or “enigmas” which fascinate both the researcher and the reader” (Katz 2001). The author thus distinguishes seven sets of assessment criteria for ethnographic reports. We will discuss only one in detail, in which the description is meant to respond to an enigma, a paradox, or an absurdity.

This is an ordinary way of questioning the world and reproducing the consistency of it, because what is at play, in the absurdity, enigma or paradox, is indeed that things hold despite all that! It is an issue also of overcoming empirical obstacles and to making them descriptive domains stimulating the curiosity of the reader, for “if the sense of mystery is not peculiar to the ethnographer, he can hope that his final explanation will also be of interest for a broader public” (ibid).

If these forms of interpolation threaten all researchers in the field, they do not always crop up suddenly during research. We must open to surprise in cultivating this floating attention which we mentioned above, and perhaps also in asking this question on a regular basis: “what’s missing in this picture?” This question is worthwhile as a variation on E. C. Hughes’ trick, repeated by H. S. Becker, suggesting that we consider that anything is possible (Becker 1998:148-150). We must precisely force ourselves to question what is given, in order not to lack important and decisive information to understand what is happening.

By way of example, take this illustration from the first field research that one of us did. The work dealt with the use of public space by the homeless in the neighborhood of Les Halles in Paris. The idea was to question the preconceived notions of the homeless as crazy or totally dissocialized people. In a Goffmanian vain, it was interesting to describe the various scenes of homelessness in Les Halles, a neighborhood which is richly endowed with shelters and distribution of material goods for the homeless, particularly dense and favorable for providing ways to get by, including several stories, corners and niches favorable to quasi-private uses of space. The issue was to understand the variety of roles played by the homeless and to tie together the stages where these roles were played. Rules, norms, values should certainly govern the relations between the homeless and their use of space.

After several months in the field, at the very moment of leaving the research site, and after reading *Tricks*, the ethnographic apprentice asked himself if, basically, nothing was missing in his observations. Then, suddenly a striking void appeared: there were no beggars in a neighborhood where the possibilities for gain seem to be numerous and accessible! More specifically, the people met in Les Halles – several dozen homeless who spent a good part of their time there – never begged in the neighborhood. This enigmatic observation reopened the field research. In notes, we saw signs of beggars but they were unknown to the homeless being studied: “Romanians” who came to beg for a few hours each day. They, themselves, they said, would never do this in their neighborhood. We had put our finger on a moral code (Anderson 1999), which proscribed certain behaviors and established forms of mutual recognition between the homeless, but also with the people of the neighborhood, particularly merchants and residents. We had discovered a descriptive domain which allowed us to draw a deeper picture of Les Halles.

Description thus appears as the most certain preliminary re(source) of the ethnographer. The ethnographic argument will be all the stronger in that it will be based on valid descriptions, according to criteria that certain ethnographers are starting to show today. Short-circuiting the description, in favor of explanations that are too quick, too cursory, too staggered vis-à-vis the practical contexts of the activities studied is a trap we easily fall into. A way of avoiding this trap is probably to cultivate curiosity and surprise during our observations, of getting involved all

the while stepping back, a step of reflexivity, thanks to our notes, index cards, memos, to put a finger on all sorts of mysteries which are woven into our research and reports.

H. S. Becker's Tricks: Polish, Specify, Clarify

In conclusion, we would like to present some advice on form which aims to simplify the text without reducing its quality or limit its scope. This is very simple advice on writing that we owe once again to H. S. Becker. We will summarize it very quickly, especially in hopes of giving the reader the desire to dive into the illustrations in the work (Becker 1986).

H. S. Becker first encourages us to adopt a simple and precise style. Clarity, simplicity and concision will protect us from two main pitfalls: the "literary temptation," on the one hand, and the "useless complication" on the other. Generally speaking, any wording here benefits from being reworked "by ear", by reading it out loud or in one's head.

The sociologist also suggests that we avoid passive constructions and free indirect style: the former fails to define the authors of the actions described ("the criminal was tried" hides the judge and the protagonists of the court's decision), without encouraging us to extend our investigations; the latter does not enable the reader to know who is the author of statements reported without quotation marks.

H. S. Becker then recommends that we cut down our texts to clarify their meaning: reducing the number of words, avoiding repetitions, means that we are required to take responsibility for the flaws in reflection, and not cover them up with vague wording. Superfluous words are often designed to protect the researcher against the risk of error: by reservations and signs of modesty, they express the awareness that we have advanced too much, or that we are not right. More generally speaking, a useless word is a word that adds nothing: "it does not refine an argument, express a significant reservation, or add convincing detail." A simple test enables us to identify useless words and phrases – skim through one's text by monitoring each word: what happens if you take it out? They are more numerous than we would think.

H. S. Becker also suggests that we make the reasoning and central mechanisms of our arguments more explicit by reducing, prioritizing, and coordinating arguments. We need to put syntax in the service of argumentation: instead of enumerating three points, we will show, for example, how the ideas are interlinked, or we will accentuate the most important idea instead of leaving it on the same level as the others. Making syntax work towards argumentation also means that we place subordinate ideas in subordinate clauses and not in the main clause of the sentence.

Moreover, H. S. Becker notes the excessive use of abstract words which most often serve as fillers: they mean nothing in themselves, but are there instead of a true idea that they help us to avoid having to formulate and specify. Thus, we need to avoid “the vague abstract” (and banish hollow words), “the general abstract” (and to deal with the case before generalizing and not vice-versa), “the abstract abstract” (and attempt to give images, examples), “the programmatic abstract” (and to do instead of saying what should be done).

We are also invited to better discriminate between useful metaphors “overworked metaphors,” these hackneyed expressions which do not add anything additional to the text. One way to test this is to systematically take them seriously by trying to draw them out all the way: does that really add something to the text?

Finally, three tricks are aimed more specifically at overcoming the anguish of writing. The first, useful to start off writing, is free writing: here we put down on paper all ideas which come to us without paying attention to the form or their implications. This allows us to dive into the water, dispelling straightaway the idea of perfect and definitive writing: the idea is to end up with a stock of notes, in bulk, which will provide material for a first pass at ordering notes, which will also ask questions about the usefulness, the relevance and the accuracy of each word (related to its synonyms, its various connotations); from one draft to the next, writing is thus continual rewriting.

The second trick concerns the construction of the outline: in making index cards, then piles of index cards, by giving them a title, classifying and reclassifying them, one can gradually specify categories, groupings of themes or close cases, and thus the possible structure of the text to come.

Finally, we can never make too good a use of the difficulties which prevent making progress in writing: it is often in describing the problem encountered, by searching for what it can teach us about the subject studied, that we advance in the construction of the problem. Just as on the research site, we sometimes have the impression that nothing is going on (Becker 2002, pp. 160-164), when we come to describe what has happened, we struggle to say that there was nothing interesting. We must endeavor to describe this “nothing.” If we judge something to be trivial, without interest, it is often because we have already incorporated the ideas of the research subjects. In looking at things from a different angle, in changing perspective, in increasing the points of view, the tasteless takes on a taste.

Conclusion

Writing is not only a mode of expression of the social world, but also of knowledge and discovery. Varying the formats or styles of writing it means that we authorize ourselves to discover other aspects of the reality studied, and to increase our rigor and reflexivity. Thus, stressing the question of writing differs appreciably from the

stress associated with post-modernism and what is commonly called in anthropology the “crisis of representation” (Clifford Marcus 1986). The issue of reflections like those of Jacques Goody, H. S. Becker or B. Latour, to mention only these three authors, consists in associating the consideration of the depth of writing, its cognitive effects and particular practices with the realistic context of the sociological approach, for which reality does not break up into the variety of its modes of understanding.

Ethnographic publication is only the last link in a “chain of writing” (Fraenkel 2001), from note taking on the fly on the research site, to versions of the text before publication, going through numerous more or less systematic inscriptions, insights, analyses, comparisons, coding. This heterogeneous and disjointed work of writing winds the ones into the others. Although the ethnographic report is not developed in the linear system of speculation and its refutation, it still claims to provide an original answer to the question “why?”. This question goes through valid answers to the question “how?” i.e. by detailed, contextualized descriptions anchored in ethnographic experience.

This last remark brings us to the readers who, at the end of the day, judge the plausibility of our writings by the yardstick of their indexation on research situations. But they can also accompany the research, as native researchers who are also authors: letters and e-mails, narratives and analyses upon the request of the researcher, diaries, without taking into account all the written signs of their practical activities (Laé 2008). The most innovative modes of writing today in the social sciences – shared blogs, fictitious dialogues (Latour 2009), cooperative ethnography (Joseph 2007), etc. – have one thing in common, to further socialize the research, by enabling the reader, near or far, colleague, well-informed native or simple passerby to participate in the field research underway.

Notes

1. We should recall the particularly deceptive expression of the French moralist Boileau: “What is well designed is clearly set out. And the worlds to say it come easily” (*L'art poétique*, chant 1, 1674).
2. Arrangement plays an important role here. With respect to its role more generally in maintaining the ability for action, the work of J.-C. Kaufmann (1991) is very instructive.
3. On the updating of historic figures and ethnographic authority: Clifford, 1983. This article was a major reference in the interpretative change in direction and in post-modern criticism in anthropology, which denounced, from both a scientific and political viewpoint, the operations of silencing research subjects.
4. (*Ibid*, p. 129-146) The reflection on de Geertz on writing is generally more sympathetic to a political anthropology which runs through his work (Cefaï 2007).
5. The interpretive change in direction in anthropology, encouraged by C. Geertz, strove to bring the author back to the core of ethnography, to reassess his place in the objectivization of knowledge, sometimes even to consider that the only thing worthy of being studied

without doing violence to the subjects of research is no longer anything but the researcher in the process of conducting it... This constructivist slope, taken particularly by the representatives of auto-ethnography, was sharply criticized because it no longer leaves the possibility of asserting anything of value on the world, and thereby strips the social sciences of its systems of scientific nature in the aid of stylistic research unsuitable for the sociological project.

6. For grounded theory, the first analyses begin even from the preparation of the first materials: "In such a way as to miss nothing that could make their saliency, the researcher should analyze the first materials as so many signs" (Strauss and Corbin 1990:366).
7. The name of this theory refers to the crash of the shuttle Challenger in 1986, due to the failure of a simple (!) toric seal.
8. M. Hammersley proposes to gauge jauger the quality of an ethnographic text by the yardstick of its plausibility and credibility. He writes, "By plausibility, I mean the degree of admissibility of an argument, given its compatibility with the horizon of knowledge that we consider as true"(1993, p. 301). He underlines the importance of the community of researchers in scientific assessment (and correction). He continues, "By credibility, I mean the degree of admissibility of an argument, given that the possibilities of error implied by the work of research seem relatively weak to us" (p. 300). In doing this, M. Hammersley relates the quality of the report to its fallibility. A good description should "give the reader the information necessary for the assessment of its validity and relevance, rather than present itself as [a definitive, irrefutable and unquestionable truth]"(p. 301). Needless to say that this fallibility is constructed during the crossing out, scribbling and resumptions of the original report.
9. For a presentation of conceptual positions and evolutions of grounded theory, see Chamarz, 2001.
10. The first version of analytical induction dates back to the 1920s and the work of F. W. Znaniecki. However, this method of data analysis has been given little notice in the French-speaking world. See the synthetic presentation of this provided by J. Katz, in "Analytic Induction," On-line at: [HTTP://www.sscnet.ucla.edu/soc/faculty/katz/current.htm](http://www.sscnet.ucla.edu/soc/faculty/katz/current.htm)
11. To become a recreational marijuana smoker, writes H. S. Becker (1986), there are necessary five stages to go through: be put in a situation of smoking by spending time with a group which regularly practices this habit, learn to smoke correctly so that the drug will have an effect, be aware of these effects (one can "be high" without knowing that one is high), link it to taking the drug, and then learn to associate these effects with pleasure. According to his observations, no recreational marijuana smoker became so without going through these stages. In linking the experience of this illegal consumption to a series of trainings, H. S. Becker thereby rejects a certain number of psychiatric and individual explanations, and in an even more convincing way given that his report on the career of the smoker is extremely fallible: by resorting to analytical induction, he makes his analysis conditional on the discovery of a single negative case, such as a recreational marijuana smoker who had not gone through all the stages described by the author.

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Far from being a collection of technical certainties and certified methods, this book interrogates the uncertain itinerary of the process of social logics discovery. In that sense, it is a decisive step towards a critical systemization of ongoing theories and practices within the African scientific community. The reader can, therefore, identify the philosophical, historical, sociological and anthropological foundations of object construction, field data exploitation and research results delivery. This book explains the importance of the philosophical and social modalities of scientific practice, the influence of local historical contexts, the different usages of new investigative tools, including the audiovisual tools. Finally, the book, backed by classical theories, serves as an invitation toward considering scientific commitment to African field research from a reflective perspective.

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